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MEASUREMENT AND MANAGEMENT

by

O. H. WANSBROUGH-JONES*

MANAGEMENT IS THE CONTROL by the few of the operations of the many. It is a widely used word, covering the control of processes in industry and commerce, education, the public services, the fighting services, research institutes, agriculture, transport undertakings.

The training received by those who become managers varies vastly. There is the traditional stamp licking or sweeping out the shop floor and working upwards. There are the cadet or trainee systems for long adopted by the fighting services and now used extensively in industry, or the selection of able young graduates or school leavers followed by "on the job" training, which is essentially what the Civil Service and a number of large firms do, or the calculated move into management by one who has a full professional training in a subject of obvious value, accountancy, law, science, or engineering. There are courses in business administration, industrial staff colleges, and the like.

All these methods have certain values. All in the end provide the necessary blend of general experience and specialist knowledge that lead to success in the management of affairs. All, in the end, lead to what is quite the most important single quality in management, the ability to deal with people, a power of command, a subject with which, for good reasons, this talk does not concern itself. Through them all a knowledge of many aids to management is gained; but

* Text of talk broadcast by Sir Owen Wansbrough-Jones on the B.B.C. Third Programme, 12 January, 1955.

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they normally put little stress on one very useful aid to management —measurement and the scientific approach.

What is this scientific approach, and how can it help management? Scientists, even when talking to each other, have considerable difficulty in explaining just what they mean by it, let alone defining it. But it certainly exists, and it is based on measurement—accurate measurement of something. It includes really knowing what you are measuring. It covers the analysis of the measurement until causes or patterns are revealed, "laws" in classical science. It means that results are never given nor statements made until the author knows they are right or rather (since the last time I said this in public I caused some concern with my meteorological friends as to whether they were scientists at all), unless the limits of probability are also stated. It means, often the setting up of controlled experiments to establish a point that is in doubt. It means listening to all facts and opinions, with an open mind, neither taking anything for granted nor excluding as untrue any opinion because it is not accounted for by any theory extant at the time.

Now it follows that if a manager of any concern used a strictly scientific approach, he would fail. A soldier who waited until he knew all the facts and never took a risk (albeit a calculated risk) would win uncommonly few battles; a Civil Servant who would take no action until all the facts were certain would be far astern of affairs; a business man who took no risks at all would soon be far outstripped by his competitors. Yet all would be the better for reducing the uncertainties which surround them to a minimum; and it is there that scientists, the scientific approach, and operational research can help them.

After all, in the broadest terms, science is directed to finding out first what happens, then how it happens and, if possible, why it happens. Operational research is, I believe, really directed to comparing what management says is to happen, what management believes is happening, and what is really happening—three different things as a rule. If you can compare accurately what does happen with what ought to happen, you may find the causes of the differences, that is, both the "how" and the "why".

This must be done by accurate measurement, which is common in pure science and in accountancy; but the sort of measurement to help management that I want to talk about is a little different from both of these.

The scientist in his laboratory can normally set up an experiment to examine the particular point he wants to study. He isolates his variables. The accountant or the statistician, who is probably dealing

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with a situation that is inherently far more complex than that which confronts the scientist (which is not the same as being more difficult), generally limits his endeavours to seeing what has taken place in the past. The operational research worker has to measure events as they take place, very rarely having the advantage of the laboratory scientist of being able to set up a controlled experiment, isolating things. He generally has to measure a process which cannot be broken down into its different stages. In fact all he can do is tap off results whenever he can and then make the most of his data—uncontrolled data at that. Fortunately, his scientific training will generally have given him special experience and facility in handling such data; or in other words, good powers of analysis and reasoning.

This brings in a further difficulty, well known to the classical scientist, but one he can normally handle. That is, the fact that every time you measure anything, you disturb it. This so-called "uncertainty principle" was originally enunciated by Heisenberg in relation to the impossibility of really knowing just what an electron was doing at any time, because you can only detect it by interfering with it in some way so that it immediately does something slightly different. It is quite general, and in operational research it may be dramatic. You can call it the "spotlight effect". Try to measure what a group of workpeople are really doing, or how traffic flows, or how a filing system operates and immediately those concerned react to the spotlight, and do something slightly different. They may work harder, or really obey the rules for once, or in certain circumstances, sulk. It all depends. Time and motion study, of varying favour or notoriety since Gilbreth and others, shows this well enough. Work study, which is different, shows it too. So do studies on working conditions. For example there was a well-known series of experiments done at the Hawthorne plant in the U.S.A. around 1927. One group of workers was used as a control. On another similar group the effect was tried of varying the illumination. On the test group the illumination was increased, doubled again, increased still further, and finally dropped down to a lower level than that at the start. Production increased in both test and control groups. Then the same sort of tests, with controls, were carried out on the effect of introducing rest periods, decreasing the length of the working day and so on. For eighteen months working conditions were steadily improved, and output steadily increased. Everything conceivable was measured. In fact the men were working under highly artificial conditions, "spotlighted". Then the group was put back to the original 48-hour week without rest periods and so on—and the increased output was maintained! The moral of this story is not obvious. It certainly does

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not prove that rest periods or good working conditions do not matter, for in many other ways it has been proved that they matter very much indeed. But it does prove, amongst other things, that before any claims are made about causes and effects, it is as well to have controls and to know, if you can know, what you really are measuring and what really governs the results you get.

Yet there are many genuine successes for measurement which can not only be claimed but substantiated. In the war radar is estimated to have increased the chance of a successful interception of an enemy bomber by one of our fighters tenfold. But a small operational research team of half a dozen scientists at Fighter Command doubled this. Again, under Professor Blackett, a team led by the late Professor E. J. Williams analysed the anti-submarine attacks made by Coastal Command aircraft and came out with the apparently improbable result that the prescribed depth-charge setting was too deep. This was so far from obvious, on grounds of experience or common sense, that it took a little while before the findings were adopted. When they were, the weekly number of kills was, as prophesied, doubled. There are plenty of other examples of the wartime use, in operations, of the concept of this scientific study of processes as they take place—hence the name, Operational Research.

But all that, and much more, was ten years ago. After the war, as attention was turned to peaceful things, much was said and written about operational research and its potential value to other than military forms of management. The British Association discussed it in 1947. An honorary degree was awarded to one of its practitioners. It was hailed as an important novel advance in social studies. Operational research groups for the Services were established on a peace-time basis. And exaggerated claims were made for its novelty and power as an aid to industry.

In 1953 Sir Charles Goodeve undertook a survey of the extent to which operational research was actually in use in Great Britain. Two hundred and ten questionnaires were sent out, mainly to private firms, nationalized industry, research associations and the like. Financial concerns were excluded. One hundred and sixty replies were received. Analysed, using a good deal of subjective judgment, they seemed to show that about a quarter of those organizations replying were in fact deliberately using an operational research unit (though less than half of them called it that), while another quarter agreed that some of their staff did operational research for some of their time. Another quarter reported the use of routine measurements —quality control, time and motion study, market research—which as routine operations and not research processes, hardly count.

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Judging from this chosen sample it seems that the general notion has spread pretty widely. Operational research is naturally to be found most in the larger organizations; but it is used as a method in such varied industries as the boot and shoe, building, chemical, confectionery, and textile trades as well as in the coal, transport and other major undertakings. There is a small but growing Operational Research Society, which holds meetings to discuss operational research progress, new techniques, and results of old ones. The Society has its own publication, and it is beginning to set professional standards. One university department at least gives periodical courses on this subject. Operational research in Great Britain, in fact, seems to have made fairly steady and healthy if unspectacular progress, and has not yet reached the point at which demand seriously outruns the supply of suitable practitioners.

In the U.S.A., however, after a slower start, it has now caught on in a way which may prove to have been too rapid. In twelve universities or comparable institutions regular graduate courses are given, and in at least two research doctorates are conferred. To deal with military operations, there are close on 1,000 operational research workers, or analysts as they are termed, and there are nearly as many again in industry. The number of operational research groups established by that name was doubled in 1953. There are at least thirteen different firms, business or management consultants, used on contract by industry to carry out operational research. Yet in spite of the large and rapid growth that has taken place it seems that about two-thirds of the companies in the United States have little or no knowledge of operational research and more than half of the firms who do use it are within the aircraft, electrical, instrument, machinery, and chemical or petroleum fields. So there is plenty of room for growth yet. And the growth seems to be at a great rate. It is estimated that it may be as much as 500 to 1,000 analysts per year, and that ultimately 10,000 people may be employed in this field. Unless this situation is very carefully handled it is certain that the quality and standard of the work will fall—and operational research, like so many other things that have been oversold, will fall then into disrepute.

This need not happen here, and it would be a pity if it did, for quite apart from the fields in which it has been used successfully there are attractive opportunities for its expansion. It is unfortunately rather difficult to give case histories or success stories, for the real successes are rarely spectacular and can often be commercial secrets. We know that the cotton industry owes much to the operational research carried out at its own research centre, the Shirley Institute.

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In the building industry it has been used to find means of judging logically and impartially the value of many new systems of building construction, in regard to prices and maintenance costs, new materials, or new equipment. In retail organizations help has been given in the constant effort to increase turnover while keeping stock down to a minimum. In the coal industry a Field Investigation Group of nearly forty members has been studying such problems as improvements in underground haulage systems, underground communications, the best ways of cutting roadways through rock, and the evaluation of certain new mining equipment, with valuable results in efficiency.

Again it has been used in studying hospital organization, nursing services, and hospital out-patient departments. Road traffic has been studied, for example, to find out what drivers really do when confronted with a road sign—a good example of the differences between what does happen and what ought to happen. While the visible presence of a policeman has a marked "spotlight effect", in his absence the number of drivers who do not stop at "Halt" signs was determined and was large. Attempts are now being made to find out just why this is—what the factors are that really govern drivers' behaviour; and how far it can be improved by, perhaps, redesigning the vehicles, the signs, or the pedestrian crossings. As in many cases, you cannot appreciably alter the major controlling factor—the driver.

Or yet again, the iron and steel trades, where very large amounts of material have to be handled, provide an excellent example of the successful use of operational research methods. The whole business of importing iron ore, and moving it up to the blast furnace bunkers is an extremely complex dynamic process, involving rates of discharge from ships, berthing facilities, turn-round times, shipping costs, to mention only a few of the variables. This is just as complicated as many biological processes which cannot be dealt with properly by straight statistical methods any more than this one could. But as a result of operational research studies it was possible to make firm recommendations, backed by facts, on the value of building ships specially designed for carrying and discharging ore, the best size of such ships, the advantages of using deep in place of shallow water ports, and the best type and capacity of discharging equipment to employ.

All these examples are taken from work in this country. There are like examples from the United States. For example, under the Port of New York Authority, studies of traffic delays at toll booths enabled the best compromise to be worked out between the conflicting objectives of economy and service. It ended, it is claimed, in

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better service with an annual saving of at least ten times the cost of the total investigation—which was incidentally a pretty tough mathematical problem. Another example is in the use of sampling methods in railway accountancy, from which it seems vast savings in accounting costs might be made. Mail order houses have to balance the extra costs of quick despatch against losses on goods returned if shipment is delayed. It took a lot of good science and mathematics to establish an average five days delay as the optimum. It was almost too difficult to work out if night opening of department stores paid. It often does not.

These are some examples of the way measurement can aid management. But which sort of people do the work, and how do they do it? What sort of special training must they have?

I think the answer to the first question is easy enough. This type of work can only be done well by good, well-trained scientists or scientifically minded engineers. It used to be held that biologists made the best operational research workers, perhaps because they were most used to handling rather intractable and uncontrolled data, but I doubt if one can generalize so far. Engineers with analytical minds could easily provide the most effective operators if only because it will often be the engineers who have to use the results obtained.

The work is done by taking measurements of events, knowing what is measured and then analysing the results. This analysis can be very difficult and involve a good knowledge of several mathematical techniques—the laws of chance and probability, the theory of games, obviously the methods of statistics, the theory of queuing on which a good deal of work is now being done; even a technique called linear programming which would be more interesting if someone could find an example to which it could be applied. These are not, in general, the normal equipment of either the experimental scientist nor the applied economist; and perhaps it is in the methods used that special training should be given. It is the methods used that mainly distinguishes operational research as a special tool.

Where can we expect to go from here? I should say that provided it is well done and not oversold, operational research has come to stay as another means of using measurement to help management. Management must learn to use it, neither expecting too much nor asking too little. As I mentioned earlier, training and selection for managerial positions results in managers of intrinsic ability, well trained and of wide experience. So it would be surprising, indeed, if the results of this kind of scientific approach often led to recommendations for the adoption of practices other than those evolved

from experience. Of a good many studies I have seen myself in one field or another, I should say about 80 per cent or more simply confirms the correctness of the methods evolved by experience. In a further 10 per cent perhaps the possible advantages offered are so marginal that the change is probably not worth making, but if in one case in ten a real improvement in efficiency can be offered, that is a very big yield on the money spent. Of all the methods of using science to help industry, operational research must be by far the least costly.

Management, too, often has to take decisions to back one process rather than another, or to make a new product to replace a well-established one, and experience is often not the only guide in a new situation. Operations analysis is then likely to correct a management's hunch on more than 20 per cent of these occasions.

So when one considers how big are the risks implicit in management decisions, and how vital the consequences, any limitations of risks at small cost ought to be welcome. Increasingly I think operational research will be welcomed.

If you can get the right people to do it, it doesn't cost much—and it works.

Vacancy

An Operational Research investigator is required by the British Boot, Shoe and Allied Trades Research Association to study means of increasing yield in cutting and sorting leather in shoe manufacture. The work will be mainly practical and experimental, but prior knowledge of leather and shoes is not expected. An aptitude for experimental work and experience of the application of statistical and other scientific methods to industrial (or service) problems are the essential requirements. Additional information will be sent on request made to Director of Research, Satra House, Rockingham Road, Kettering, Northants.

An operational research centre "Centro di Ricerche Operativa" has been established at the University Bocconi, under the directorship of Professor Francesco Brambilla. The address is: University Bocconi, Via Sarfatti 23, Milano, Italy.

The séminaire de Recherche Opérationnelle de l'Institut de Statistique de L'Université de Paris has recently published its first Bulletin for members of the Group. It is noted among our abstracts. The Secretary of the Group, Mr. André Leroy, 11 Rue Pierre-Curie, Paris (5*e*) would be very pleased to hear from members of the Operational Research Society if they are staying in Paris.

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THE ACCURACY AND USE OF TIME STUDY

THE OPERATIONAL RESEARCH SOCIETY'S open meeting on 20 December, 1954, took the form of a discussion of the above subject. It was introduced by papers given by Professor T. U. MATTHEW, Department of Engineering Production, Birmingham University, and WINSTON RODGERS, Department of Scientific and Industrial Research. An abridged account of the proceedings follows.

T. U. Matthew

OPERATIONAL RESEARCH on time study practice requires the investigation, measurement and analysis of the total situation in which use is made of time study as an industrial engineering technique. It is not sufficient to test the quality or consistency of rating or of time study results obtained by individuals or by groups of time-study engineers among themselves, without reference to the uses being made of time-study values and of the corresponding levels of quality or consistency required by management and workers in the departments or industries represented in the groups.

The use of time study in British industry is subject to a number of well-established checks and controls, which ensure its continued acceptability by management and workers as a trusted technique for determining the standard times to be allowed for the performance of work using previously improved methods. A study of the operation of these checks and controls constitutes an important part of operational research on time-study practice.

An important use of time study from the productivity improvement standpoint is in conjunction with method study directed towards the elimination of unnecessary work and effort. Job times based on time study are also used for balancing work loads between individuals and departments for production programming and for control of time lost due to various causes. For management purposes of this type, speed may be much more important than extreme accuracy in time study.

Where time study is used as a basis for piece-work or other incentive schemes, however, a high level of consistency and accuracy becomes essential. One of the most important traditional British checks on the quality of time study is the agreement that time study values or "allowed times" may be applied only with the agreement of the workers concerned with the task measured, and it is always open to the workers to challenge a particular value at any stage.

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In a sample of firms, the percentage of values challenged by piece-workers over a six-months period ranged from 0·3 per cent. to 0·85 per cent. of the time values issued, and in less than a third of these cases was it found necessary to make any adjustment to the values, after re-study. This evidence supports the conclusion that where suitable checks and controls are exercised by management and labour the accuracy and consistency of time study can be brought within the acceptable limits within any given plant. Studies on performance rating carried out by the Work Measurement Research Unit at Birmingham University indicate how this situation may be further improved by the individual time-study observer, by time-study sections within the plant, and by nation-wide industrial groups. Having regard to this total situation, to the possibilities for further improvement in the control of quality of time study which exist, and to the potential advantages to both management and labour in the use of time study as a productivity improvement tool, it is not surprising to find that time study, and other work measurement techniques based upon time study, are coming increasingly into use in industry.

The Human Relationship Framework

The wider use of time study makes it more important that all possible steps are taken by management and by time-study engineers, within the individual factory, firstly, to establish a stable human relations situation on a basis of agreed industrial relations practice, and secondly, to maintain the quality of time-study work within acceptable limits by the use of well-devised and suitable controls.

As positive steps towards stabilizing human and industrial relations, management and time-study engineers normally give guarantees, based upon agreement:

(i) regarding the average level of earnings which will be attainable in the factory by piece-work operators working on time-study values from week to week (e.g., 133 per cent. or more of basic or consolidated rate),

(ii) regarding grievances procedures (e.g., on disputed time values and lost-time allowances and on provisions for check-time studies),

(iii) regarding the conditions under which time study will be carried out (e.g., openly—on agreed samples of work and/or on agreed operators),

(iv) regarding circumstances under which established time values may be altered (e.g., where changes of method or of working conditions occur, or where errors in computation have occurred).

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The quality of time-study practice can be viewed in perspective only in relation to this framework of human relationships existing within the individual factory, regarded as a close-knit living entity.

The Quality of Time-study Practice

The term "quality" in time study may be defined for the purpose of measurement, analysis and control, in a number of ways; but the three most important measures are *accuracy*, *consistency*, and *acceptability*.

First of all, the *accuracy* of a time study should be defined in terms of what the time-study engineer sets out to do. This is to establish within acceptable limits the standard times to be allowed for carrying out defined tasks at a "standard" level of performance, which for the tasks concerned may be maintained indefinitely day-in day-out by normally skilled workers without ill effects or difficulty. Until physiologists are able to provide data for the determination of absolute standards of performance for industrial tasks, the time-study engineer is obliged to use his subjective judgment for the "performance rating" of observed times. It should be noted that performance rating cannot in fact be avoided, for even in cases where standard time values are determined by averaging the actual times observed, the time-study observer and the worker must have agreed, actually or implicitly, that a standard level of performance had been maintained during the study.

It is sufficient meantime, for measurement and control purposes, to define the "correct standard time" for a task as the average value determined by all qualified time-study engineers.

The "accuracy" or "error" of an individual time-study engineer may then be determined by reference to this average. If his concept of standard performance is too high or too low compared with the average, his allowed times will be correspondingly "tight" or "loose" compared with the average. A time-study engineer's concept of standard performance may be accurate, measured over a number of different jobs, but he may have tight values on some and loose values on others, compared with average. Similarly, a time-study engineer may not rate changes in performance level proportionately during a study although his standard may be correct.

When "allowed times" are used in conjunction with suitably devised incentive schemes, normally skilled individuals, or groups of operators in a factory, on incentive should be able to earn, on average, the guaranteed or agreed extra amount.This does not mean that all operators will in fact earn the average extra amount—some will wish to earn more and some will be satisfied to earn less than

the average, but it should be possible for all operators of average skill and ability to earn this amount without undue effort or any ill effects. This possibility must be self evident if time-study practice is to remain acceptable from the workers' point of view.

As a week-by-week control on *accuracy* and *consistency* most chief time-study engineers or plant managers maintain some form of statistical control at factory level and departmentally, and special attention is given to exceptionally high and low earnings and to any change in the average level of performance or in the character of the distribution. Individual operator earnings and performances are also studied in the same way and are control charted on the same basis.

The quality of time study may be measured also in terms of *acceptability* in relation to the cost of the time study service. We are concerned with the acceptability of time values to both management and workers. Few managers or workers are prepared to see the average earning level fluctuating widely from week to week or to see the distribution of incentive earnings change suddenly without good reason. Also, disputes over unacceptable values are costly and are not sought by either managers or workers.

The controls used here are related to the grievance procedure agreed upon. It is significant that in most factories studied no records are kept of either the number of new values challenged, restudied and agreed, or of the number of old values challenged, restudied and agreed, where changes have taken place, since any questions raised by operators are generally settled directly on the shop floor. Investigations based upon records specially kept during the investigation of this aspect, show that the number of challenged and disputed values is remarkably small in most factories. Table I illustrates this point, in which the extent of the changes made and accepted also gives some indication of the accuracy and general

TABLE 1

	Time-study values issued	Values challenged (over six months) New values	Old values	Number Re-studied	Number of values adjusted after re-study	Range of Adjustment in value
Factory A (627 direct workers)	9,234	29= 0·3%	7	16	10	+4% to +6%
Factory B (200 operators)	1,653	14= 0·85%	—	6	2	+3% to +5%
Factory C (500 operators)	1,200	6= 0·5%	—	2	1	+2% to +4%

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consistency of time-study engineers in these factories since operators were satisfied with changes of as little as 2 per cent. and 3 per cent. on re-study.

The Improvement of Time-study Practice

Although the evidence obtained from individual plants suggests that time-study engineers operating under present checking and control procedures are in fact determining the allowed times for tasks within limits of accuracy and consistency which are broadly acceptable to both management and to the workers to whom the times apply, there is no reason for complacency with the present quality of time-study practice. Professional production engineers and time-study practitioners are fully conscious of the need to improve practice further. The first report of the Joint Committee of the Institution of Production Engineers and of the Institute of Cost and Works Accountants on "The Measurement of Productivity" drew attention to this need and quoted wide differences which can occur when comparisons are attempted between the allowed times established for the same operation by time-study engineers from different plants[1].

For this reason, experimental studies have been carried forward by the Work Measurement Research Unit of the Department of Engineering Production at the University of Birmingham over the past five years. These have been directed first of all to determine the accuracy and consistency of performance rating of time-study engineers from different plants, towns and industries throughout the United Kingdom. Secondly, studies have been carried out on the subject of relaxation and other allowances of time customarily made in time-study practice. Finally, attention has been given to the measurement of acceptability, as mentioned above.

The results of the studies on performance rating may be generalized as follows:—

1. Observers from the same plant.
   - *a.* There is a basic variation of inconsistency of the *individual time-study observer* from study to study of $\pm$ 1 per cent.
   - *b.* Variations among *different observers in one plant* is $\pm$ 4 per cent.

2. Observers from different plants.
   - *a.* Variations between observers from *different plants in the same town* is $\pm$ 1 per cent.

b. Variation between observers from *different plants in different towns in the same district* is $\pm$ 1½ per cent.

c. Variation between observers from *different plants in different towns in different districts* in the United Kingdom is $\pm$ 2 per cent.

The percentage variations stated above are standard deviation figures derived from the analysis of performance ratings made on films and shop floor tests by engineers drawn from plants in different firms in all parts of the United Kingdom. About 60 per cent. of these time-study engineers came within the range of variation quoted, and about 95 per cent. of these engineers came within twice the stated range of variation.

From this analysis of the factors affecting rating accuracy, the action required to improve present practice can be stated in order of priority as follows:

(a) *The Individual Time-study Observer.* Variations in times established on the same operation on different occasions (or in rating from observation to observation) may be minimized by the individual observer himself:

(i) by checking the self consistency of element times and performance ratings using graphical or other simple statistical methods;

(ii) by increasing the number of cycles of work studied in proportion to the variability of element times and ratings and to the level of self-consistency required;

(iii) by training and practice with other observers.

(b) *The Group of Time-study Observers in a Single Department or Plant.* Variations between the times established on the same task by different individuals in a single department or plant can be minimized by management action:

(i) by setting up plant standards of performance for typical tasks;

(ii) by systematic training of observers using these standards;

(iii) by instituting cross-checking procedures between engineers, and using previous values and tables of predetermined element times as an additional check;

(iv) by more than one observer studying each task;

(v) by instituting periodic reports on acceptability of values and on the distribution of earnings.

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(c) *Professional Time-study Engineers throughout the Country.* Variations between engineers on the same tasks in different plants usually remain unknown except where large industrial groups or Trade Unions may attempt to make comparisons of times allowed as a basis of productivity measurement or for other purposes.

It is only possible to control and minimize this type of variation, which is influenced by local factors and traditions in many cases:

(i) by setting up company group standards of performance on a national basis;

(ii) by systematic training and checking between engineers in different plants using these standards.

Outside industrial groups, action in this field would require to be carried out with the help of the British Standards Institute.

Conclusion

In time-study practice the variations likely to cause most disputes are those arising from individual inconsistency and between engineers within a single plant.

Studies on acceptability suggest that the operator will dispute values only when allowed times are "tight" to an extent affecting his incentive earnings. In practice he may then agree to accept a new value differing from the disputed value by as little as 2 to 3 per cent. This provides some evidence indirectly, which suggests that the operator is able to rate standard performance at least to the same order of accuracy as the time-study engineer, when the latter is working under good conditions.

In view of the increasing use being made of work study in British industry, it is important that plant managers and time-study engineers should re-examine the levels of accuracy and consistency maintained by time-study staffs within individual plants and industrial groups. Present industrial trends are to extend the field of application of work measurement based on time study, into indirect and craft work, and it is particularly important that the quality of time study should be maintained on a high level, and variations in standard times minimized using some or all of the methods outlined.

Increased productivity depends upon improved methods and also upon accurate measurement of work. It is important to ensure that the methods of measurement remain acceptable.

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Winston Rodgers.

THE SPEAKER DESCRIBED and discussed an investigation into the consistency of stop-watch time-study practitioners sponsored by the Nuffield Foundation and carried out at the Acton Technical College. The investigation had been fully described in a publication[1] earlier in the year, and will not be repeated here. Mr. Rodgers expressed regret that in the long interval between the investigation and publication of its results various ill-informed comments had been made, and welcomed the present opportunity of discussing the work. Further investigations were now being carried out and the lessons drawn from the earlier work could be regarded as interim conclusions.

These were generally as follows:

A graphical analysis of observer's elemental ratings[2] against time does not show any body of agreement amongst observers either on choice of elements or on appropriate rating variations between such elements.

A graphical analysis of the average times taken by operators for a cycle of a task plotted against the day and session suggests that the operators improved and became quicker as the experiment proceeded.

The data for assembly and machine type operations are not amenable to quite such detailed analysis as is possible for a solution operation as used in the investigation.[3] Nevertheless, all the results that have been established for these first two tasks, agree remarkably well with the corresponding results for solution. Thus it seems fair to quote results applying in common to all three tasks, even though in a few cases such assertions have been established for solution only; and this is done below.

None of the following factors have shown any significant influence upon observers' inconsistencies:

(a) which operator was studied,

(b) whether the study was the first or the second of a pair of successive studies,

(c) whether or not standard elements were used.

This is not to deny that these factors have any influence; the assertion is merely that, whatever influence they may have, it is swamped by other dominant sources of error. If time study were more exact, this conclusion might no longer hold.

On the other hand, there is strong evidence that on average an observer agrees with himself more closely than he does with his mate from the same firm,[4*] and that in turn observers from the same organization agree with each other more closely than they do with observers from other organizations.

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In the following paragraphs the degree of inconsistency between observers is described in terms of standard deviations of percentage variation. All such figures must be interpreted accordingly. See page 14, lines 6 to 12 in Prof. Matthew's paper.

There is 7 per cent standard deviation in the variation of times taken by the operators to perform an average cycle of a task, this variation being in part due to differences between the performances of different operators and in part due to differences between performances of the same operator on different occasions. This figure represents the variation which occurred in this experiment under laboratory conditions: there are no reasons to suppose that it would remain valid in other circumstances. Of this 7 per cent, 5 per cent standard deviation arises from differences in operator performance between the two studies of the pairs of successive studies, studied by the same observer, that is to say within such pairs; and a further 5 per cent standard deviation arises from differences between such pairs. (It is worth noting that here, as elsewhere, the combination of two sources of variation is not by straightforward addition; and that two separate components of 5 per cent each combine to give an overall component of 7 per cent and not 10 per cent.)

If all observers were able to rate perfectly they would, in producing their final figures, compensate entirely for the variation of 7 per cent noted. If each observer had a perfectly self-consistent method of rating, but different observers had different methods of rating entirely inconsistent with one another, the observers in the experiment would compensate entirely for the 5 per cent variation within pairs of successive studies, but would leave the second 5 per cent between pairs uncompensated. The analysis shows that this second 5 per cent has not been compensated in any way, whilst 55 per cent of the first 5 per cent has been compensated. The immediate conclusion is that individual observers are 55 per cent self-consistent at rating, but that they are wholly inconsistent with one another. But this immediate conclusion is not necessarily the meaningful conclusion. Self-consistency does not necessarily imply that the observer is able to appreciate operator's speed: it merely means that his figures for that are self-consistent, and it is not inconceivable that this is due to subconscious or even deliberate adjustment of them.

The standard deviation of the variation in the observer's final figures[5] within the pairs of studies made by the same observer is 3 per cent. This 3 per cent variation consists almost entirely of what remains of the first 5 per cent (discussed earlier) after reduction of 55 per cent rating compensation; all other contributory sources of

variation are effectively negligible. If one assumes that the observers in the experiment were typical of study-men in general, one may infer that this figure of 3 per cent standard deviation measures the variation to be expected in a given typical observer's final figures (that is to say, it measures the self-consistency of study-men). Two provisos apply: firstly, this percentage might be lowered when study-men in industry habitually take more than two studies of a task; secondly, the percentage would presumably be increased (though not necessarily in strict proportion) if there were an increase in the percentage variation of operator's speeds.

Consider next the variation which one may expect between the final figures of similarly trained observers.* This variation will consist partly of the 3 per cent arising from individual observers' self-inconsistency, and partly from inconsistency between the similarly trained individual observers. The latter source contributes a standard deviation of 8 per cent, due almost entirely to inconsistencies (between observers) in the allocation of allowances and rating figures (as would be expected from observers who cannot appreciate operator's speed but allot their ratings more or less at random within a certain band of values characteristic of their method of training or organization). It is difficult to break up this 8 per cent into two components due to allowance and to rating variations respectively; for these two sources seem fairly strongly correlated (coefficient of correlation $= +0{\cdot}7$), in that the observer (from a given organization) who rates generously also tends to give generous allowances. The two components of 3 per cent and 8 per cent combine to give a total variation of 9 per cent standard deviation between final figures of similarly trained observers.

Finally consider the variation to be expected in the final figures between observers in general. There will be 9 per cent variation between similarly trained observers. To this must be added 19 per cent standard deviation due to variations between different organizations (or methods of training). The larger part of this 19 per cent, say five-sixths of it, is due to inconsistencies (between organizations) in their methods of rating, allowances and company policy. It is difficult to separate these last three components from one another, but it does appear that the organization which is generous in its rating tends to be strict in its allowances and company policy (coefficient of correlation $= -0{\cdot}4$). The 9 per cent and 19 per cent combine to give an overall variation of 21 per cent standard deviation.

This figure of 21 per cent is of greatest interest because it measures the error which one would expect from a typical observer

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drawn from the aggregate of all study-men of various creeds and organizations. Or it can be interpreted as measuring the inconsistency within such an aggregate. One commonly hears time-study spoken of as being a procedure accurate to about 3 per cent; this would imply a standard deviation of $1\frac{1}{2}$ per cent. The inference is then that this experiment has shown that time-study is fourteen times as inaccurate as it is commonly supposed to be.

* Biographical details of observers suggest that although they were employees of the same firm their training in time study was not necessarily similar.

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[2] *Op. cit.*, p. 6.
[3] *Op. cit.*, p. 2.
[4] *Op. cit.*, p. 3.
[5] *Op. cit.*, p. 6.

Discussion

The CHAIRMAN, A. W. SWAN, said that the meeting had heard about two sets of work on the subject under discussion. There were others, and they included one which was not at all known in Great Britain, a Belgian effort which was very similar to the work by Professor Matthew and owed a good deal to him.

Several people in the United States had done work on the subject. Professor Mundell, who had spent some six months in Great Britain with Professor Matthew, had published work on the subject. Adam Abruzzi, another American, had done work on a rather different line, having looked at time study more as a statistician, regarding variability and the causes of variability. An American consultant, L. A. Sylvester, had also attacked time study from the statistical point of view, variability and distributions.

In Great Britain there was work by the Building Research Station in the form of a very interesting paper by Kinniburgh and Vallance which again looked at the matter of consistency. In a small way he himself had thought about time study and looked at it from the point of view of the statistician. It had nothing to do with rating. He had been looking at steelworks mill studies, and it seemed to him that the form of the distribution mattered a good deal and that one could deduce a great deal from the actual shape of the distribution, often without having recourse to more refined statistical methods. Some of that work had been published in *The Engineer*.

Mr. GOULD (Unilever) said that there was undoubtedly variability or inconsistency in values arrived at through time study by

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various practitioners. Those who had been in the profession for many years had recognized this for some time and had tried to rectify it by taking steps in various ways in the realization, first, that the basis of training was vital; secondly, that the type of work in which those men were trained was also vital; and, thirdly, that some routine exercise or discipline was necessary in order to maintain a consistency in rating between practitioners, rating being the essential factor which was responsible for most of the variability. He was not unmindful of the inconsistency that one also found in allowances, but he thought that that could be dealt with, and there were, indeed, various ways of dealing with it.

He thought that the practice of measurement—time study, as they called it—had given them variable results in industry and in various ways, but in the test of experience it worked. He suggested that there was something fundamentally wrong with the way the tests were devised since they did not, in general, add up to what was found in practice.

He suggested that until they could arrive, through various scientific means, at a basis for devising what was a man's capacity in relation to work of a particular type, both physiologically and psychologically, they were bound to get variations, not only between individuals but also between factories, districts, nationalities and countries, because they were not measuring back to the same basic concept. He could not describe that concept in scientific terms but he would describe it simply as the ability of the individual in the circumstances to give what was described as a fair day's work for a fair day's pay—it was as broad as that. The various methods and devices used to obtain that assessment had worked in practice.

He would welcome some comment from the scientific world as to how one could set about arriving at methods of ensuring greater uniformity or consistency among practitioners on both a local and a national scale.

Dr. T. E. EASTERFIELD (D.S.I.R.) said that Professor Matthew had referred to accuracy and acceptability as well as consistency, and had pointed out that by and large the results were highly acceptable. In the few cases where adjustments were needed, the adjustments were usually very small. That appeared to suggest that the results were fairly accurate. But the fact that variability was far higher than the adjustments made suggested a strong self-norming element which might be investigated.

How far was the operative, the foreman or the manager capable of telling to within quite a large factor what was the usual sort of

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normal performance? If the rate were a little loose or tight might it not be taken up by a slightly slower or faster pace of work? As long as that happened he did not believe that it would be possible to tell what was really the normal performance of a person which could be expected. He was not sure that it was necessary that it should. Probably what one needed was something which would be accepted by both sides and would by and large tend to increase efficiency. If one had that, even though it had little relation to absolute standards but was accepted by both sides, it was a good thing.

Papers which suggested that the authors were trying to get to the absolute standard tended to come down to what one would get as an average of a lot of observations by a lot of observers on a lot of objects. Dr. Easterfield quoted work by Stansfield and Scott and Linton which tended to show that what was considered a fair day's work for a fair day's pay was very largely a social matter.*

It was possible that there were in some cases ways of telling the level beyond which one must not go—in telephone exchanges during the war it was found that above a certain number of calls per operative the exchange broke down—but what was wanted was a social study of how the time-study man worked and fitted into the factory or whether his activities were mainly useful for some valuable by-products which no one had realized. It might be that he could solve dispute which would otherwise be a matter of argument, and that might be of value even if his arguments were wrong.

Professor MATTHEW, in reply, said that both Mr. Gould and Dr. Easterfield had touched on the same point, which he had made at the outset of his contribution, that time study had to be judged in its own context.

The *accuracy* with which time study operated was measured in terms of what the time-study engineer set out to do. He set out to fulfil certain conditions. He himself, using data collected from the situation itself, had attempted to show that the time-study engineer fulfilled the conditions with surprising accuracy and that accuracy did not happen to coincide with the *consistency* which he managed to achieve on a rating test, which was quite a different thing.

The same thing was true of *acceptability*. To some extent, that was a study of a complete social environment. Although engineers were doing it, and although they were making a very good job of it, he felt that it might properly be the field of the social scientist. Perhaps there was a pragmatic element about it, but there was always

* An interesting and detailed comment on this point in another context can be seen in Dr. Daniel's paper in this issue, page 38.—*Ed.*

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a desire on the part of the scientifically minded person to get down to some fundamental unit measurement. He had already challenged the scientific physiologists to produce results which were as accurate in their own terms of reference as time study in its terms of reference, but they would have a difficult task in trying to do that.

The question of *consistency* must take second place. If they regarded accuracy detachedly, within the framework which they had set up, they found that their observations coincided with what occurred in practice. Time study was being more widely used to-day than ever before, and this was because of its underlying acceptability and accuracy.

M. J. MORONEY (Unilever) said that what Professor Matthew had advanced as a test of accuracy was no more in itself than a test of acceptability. Examination of performance following the time-study engineer's work might only be answering the question "How well is the operative covering up for the time-study engineer's inaccuracy?"

Acceptability, he thought, was very much a social matter. In the pottery industry, after one substantial rise in wages, the wages bill for the industry had returned to its original level within a month or so, with a drop in productivity.

R. SOLT (B.I.S.R.A.) said he agreed that it seemed that it was purely a test for acceptability and not one for the consistency of time study. But if one took that in conjunction with the other results—which showed that inconsistency existed—one had to ask whether taking the two results in conjunction did not add up to the fact that, since objections would be raised by the operator if the time-study rating was too tight, time studies were almost always very much on the lenient side. There were no objections when time study was too lenient; the operators accepted it and worked to a level of output which gave them the earnings they wanted.

Professor MATTHEW replied that time-study engineers had a double task. First of all, they had been given the task of maintaining accuracy and acceptability on the terms which he had defined. They had also been asked, but not quite so precisely, to achieve a high standard of consistency, which was the safeguard against the situation which Mr. Solt had mentioned. In other words, it was very easy to have a situation in which both sets were accurate but in which there was no consistency; but there was a place for both.

The fact was that that afternoon they were concerned with the accuracy and use of time study. The main use of time study was to bring in management control. The second aspect of time study among different operators and different operations was subservient to that.

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Therefore, the management control aspect called for accuracy and acceptability, and only to some extent did it call for consistency, and that was what the time-study engineer was giving.

J. G. H. PHILLIPS (I.C.I. Ltd.) said that accuracy and acceptability were clearly different. In fact, the question of accuracy in the circumstances outlined by Mr. Solt would show up very clearly. If a group of operators were restricting their output, one would not get a normal distribution of performances. If one got the normal distribution of performance and the curve was somewhere about where one probably wanted it to be, one had the accuracy required.

There was another point in connexion with consistency and rating. His organization expected a man to earn a 33⅓ bonus when he was doing his best; that was when, from the point of view of the organization, he was doing 80 work units per hour. Another organization might well expect a man to earn only 25 per cent bonus when he was doing his best. If a different rating scale was being used, that could be reconciled later on. But if, instead, the organization was using what it said was the same rating scale, it might well regard 75 in the same light as he regarded 80 and there might appear to be an inconsistency in the rating which did not really exist.

The CHAIRMAN suggested that the terms "accuracy" and "consistency" were being confused.

He drew attention to the fact that time study was not exclusively used for the purposes about which they had heard that afternoon. It could be used, as he had used it, for dealing with plant capacity, and in heavy plants such as steelworks that was a very important use. Accuracy—he was using the word carefully relating to actual time study, no rating coming into the picture—mattered a great deal.

At Courtaulds' the operation research and the work-study departments were using time study jointly, as a study of working conditions, with particular reference to machine interference, which was a very important problem in all textile fields.

M. J. MORONEY (Unilever) said, with regard to the difference between "accuracy" and "consistency", that "accuracy" to him meant whether the measuring rod one was using was of the right size. "Consistency" meant that, whether or not the yardstick were of the right size, the results were not too subject to haphazard causes of variation.

Apart from "consistency", Professor Matthew had claimed specifically for "accuracy", and held up a graph for those present to see saying, "This is the measure of accuracy". His own contention was that it amounted to measuring one piece of elastic with another piece of elastic.

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Professor MATTHEW said that Mr. Phillips had put his finger on the point; a study of performance could tell one a great deal about whether the normal level or the average level for the population from which the figures were derived was in fact the normal or above or below the normal. That was exactly how that control, which could be used departmentally, sectionally or individually, could be applied.

The other aspect about the test of accuracy, which Mr. Moroney had called the test of acceptability, was that if one compared the units within that distribution—distribution sectionally and individually—with the total distribution, one got a great deal of information about accuracy.

Dr. T. E. EASTERFIELD (Department of Scientific and Industrial Research) said that the description of "accuracy" originally was its relationship to the sort of basically good performance that one would have with correct operators. By studying the distribution, one got a picture of whether the operatives were normally distributed and where one's standards tied up with the average for that distribution. It seemed to him to remain entirely unproven that that bore any relationship to what people might do under hypothetical circumstances.

It was reasonable to say that the norm was the norm that one got with the present lot of people that one had or with the present people in the country. But he thought there was no evidence to show that one could go on to claim that one was being accurate in the sense that it had some sort of relation to what in some moral sense was a fair day's work on the basis of what a perfectly trained person could do. It was that which had given what was a very valuable procedure a strong air of mystification.

It was no doubt true that if one kept careful statistical records of the performances of workers one might get evidence of the cases of restriction of output, but on the other hand when it was merely a case of taking a little more or less time over it and between jobs, it seemed to him that one would probably want a very deep statistical investigation to uncover it, and the number of firms where that would be possible would be small.

He was by no means as certain as Professor Matthew was that evidence of restriction of output was likely to be shown up and eliminated by the regular activities of the time-study man.

T. P. HUNT (Philips Electrical) said that a good deal of what they had heard seemed surprisingly unreal, both from the lecturers and those taking part in the discussion. As the first speaker in the discussion had said, in spite of all that they had heard, the method

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worked. It was a reliable tool. His organization had found that it could give its time-study engineers a test which was strange to them all, and their results normally came to within ±5 per cent and sometimes a good deal less.

Mr. Hunt emphasized that the job of the time-study department was not to set the standards for the factory, but to give a consistent guide so that the management could compare the past with the present and set its standards for the future. The time-study department was not saddled with the responsibility of ascertaining what was an absolute standard of a full day's work. Consequently, what was wanted was a consistent standard above all things.

Mr. Hunt asked if Professor Matthew could tell the meeting anything about the success of post-war experiments in Germany, where there was a national bureau to set the standards for time study for the whole country. Were those standards consistent or equal over the whole country, and had they remained consistent or equal over the few years during which the experiment had been running? He suggested that all that was required was consistency within the individual firm.

Professor MATTHEW said that there was in the audience a member of the German organization for time-study training, and it might be appropriate if he were invited to say exactly what the standards there were and how they were maintained.

Dr. M. KNAYER (German National Time Study Centre) said that the time-study people in Germany were trained in particular by judging the speed of walking. The German labour unions considered 4·0 km. per hour a normal standard, but they themselves considered 5 km. as normal, so they already had a piece of elastic. The British mile was equal to 1·5 km. and three miles was considered normal. The British figure was thus in between the German figures.

In a simple operation like walking one expected from a good time-study man an accuracy of ±5 per cent. With complicated operations it was different. They found that little by little they began to make motion study, and they knew that the method was much more important than the rating. That was his official opinion.

He wished to say a few words in a personal capacity. Perhaps they were trying to measure something with a higher accuracy than could be obtained. Suppose one had a resistor of 100 ohms and an instrument that went to 0·1 per cent with which to measure it, and the resistor was changing its resistance. What would be the value of the measuring instrument which had a variation of ±0·1 per cent if it was a hot day or a moist day and the resistance of the resitor was uncorrected, and it varied by ±5 per cent? One would no doubt

believe that the instrument was wrong. But what they in fact called the normal performance might be varying from operator to operator. It was almost a miracle that they were able to achieve such a high degree of accuracy in spite of the variations between operators.

C. J. ANDREWS (Hoover) disagreed with Mr. Hunt. Management employed time study to set standards. He criticized the tests of consistency applied by Professor Matthew, in which he himself had participated. The time-study engineer should familiarize himself with the job before he started rating it, but in Professor Matthew's test, the first they knew about the jobs was when they were flashed on the screen, and they saw them for about half a minute. That method was bound to give inconsistency. If Professor Matthew had taken a firm's time-study engineer on one of the firm's jobs with which they were familiar, the inconsistencies would probably have been reduced to about ± 3 per cent instead of ± 13 per cent.

Mr. ANDERSON, however, said that the results had been analysed to ascertain whether there was any improvement in rating throughout the period of the session. There were five shots of the operations, and tests were carried out to determine whether the first shot was worse rated than any of the others. There was, in fact, no difference between the first and any of the others.

A certain amount of time was spent in describing the operations. Stills were shown of the operation, and the operation was also shown in film form. The operations were simple ones, such as dealing cards, picking up two marbles from a tray and putting them through slots, and a simple drilling operation.

They had also taken studies made by operators on jobs which they were doing in their own factories, and they had found precisely the same inaccuracy or inconsistency as had been found in the other studies. The fact that the results of Mr. Rodgers's work and the results of the work carried out by the research unit were fundamentally the same, seemed to validate the use of films for investigation.

KEMBLE COOK (Production Engineering Ltd.) questioned whether Mr. Rodgers had been guilty of a lack of precision in criticizing time-study people. He could not see in the paper a precise definition of what concept Mr. Rodgers was using to judge the consistency of time study. At the beginning of his paper he talked about taking times and adding a certain allowance and he said that in that way the standard time was reached. Later on he listed a lot of allowances, which included "company policy" and something just called "allowances", and he said that that gave a final figure which was the standard time.

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He did not think that Mr. Rodgers had really defined his "piece of elastic". The result would be a particular answer for a particular company. He wished to inquire how Mr. Rodgers had expressed to the people who had joined in the test what it was that they were looking for or on which they were being rated. Was it a level of working of 60, 75 or 80? Did they know to precisely what they were being tested?

Mr. RODGERS replied that it had not been for him to provide rigid or elastic standards. The people who took part in the test were asked to provide an answer to the question "What would be the time allowed for the performance of this task in your factory?" Whatever the norm for the factory was, that was the answer which they gave.

Allowances varied in different companies. They were, therefore, recorded in the time-study sheet where they could be grouped together and then taken out as outside the control of the time-study men. If a company decided that an operator should earn 60 per cent or 40 per cent, it was felt that that was not something for which the time-study engineer was responsible. It was a figure which he had to include. Where possible, it had been extracted, but where the allowance was genuinely something which the practitioner himself, as a result of his own experience and judgment, included, that was given in a separate column. The time was the time they would have allowed for a particular job relative to the norm in the factory.

Professor MATTHEW said that he had taken some pains in introducing the subject to try to distinguish between what the time-study engineer was attempting to do and the consistency that he attained, when it was put through the various rating and other tests. The two were quite different things.

The rating tests that were carried out were carried out for a specific purpose, to measure as objectively as possible how consistent rating could be. That was quite different from the study which they had since gone on to make of how consistent time study could be in practice in a total situation. They had satisfied themselves that, although they used films for the rating tests, they were obtaining data on rating policy and the psychological processes involved in rating. They satisfied themselves by getting engineers from different plants in which there were three, four or five engineers, to carry out studies on the shop floor in their own way, and from those studies and shop-floor tests they obtained identical data on rating to what had been obtained by the use of films. They were satisfied that they had a measure of the adjustment.

It was evident that the great worry in the meeting was the question whether the time-study engineer was using a piece of elastic

as his concept of normal performance or whether his mental judgment of what constituted a normal performance for a task was something which was fairly constant. It was clear that a standard or normal level of performance had to be defined before they could get any consistency between observers. If the company was a nation-wide one and had plants in different towns, it would be very much concerned with setting up a national standard to obtain consistency among its plants so that it could make cost comparisons and so on. Therefore, they were involved in the situation of setting up standards of performance. The companies which had done so should not expect the time-study engineers who knew those standards of performance to achieve much better results than they obtained under the national survey of rating.

As he had shown on one of the slides, they found that where groups of time-study engineers were trained on established standards of performance, they could achieve surprisingly accurate results which compared with any other form of results and measures of performance which could be used by physiologists or any of the other so-called exact sciences. So the method of the subjective element in time study was not to be feared as it had once been. That was subject to training and control.

G. H. L. ANDREW (British Nylon Spinners) suggested that in making a national survey one should not try to isolate the consistency of time study or the consistency of rating without taking account of the base rate paid in the particular firms from which the observers had come.

He had two companies in mind, both of which were doing excellent work study. In one the "national 80" is rated at about a 75 and as a 90 in the other. The normal bonus paid is about 25 per cent and 45 per cent respectively. But these differences are resolved when we find that the base rate in the first firm is deliberately some 10 per cent above the local level while the second firm is paying its bonus on a "calculator" which is only about three-quarters of the base rate and is using a "pay curve" which is not straight-proportional.

The generally accepted "incentive pace working" is the common one in both firms and in practice the workers do receive a bonus of about one-third above the full normal district base rate despite the apparent discrepancy in rating standards. Isolated from base rates and payment curves men from these two firms would show very misleading differences in a rating survey.

Furthermore, in drawing implications from the survey, we should remember that some companies have to pay high wages to get the required labour—if a firm finds it convenient to do this by

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use of a special rating scale which gives high bonuses rather than by alteration of the base rate, this should not be interpreted as an error of rating so much as a special managerial policy. In general, he felt that many variations in rating standards originated from external pressures, such as supply and demand of labour, and although one might not consider this manipulation of rating standards to be good work study policy, nevertheless we should not confuse it with inconsistencies in the judgment of rating technicians.

He put two questions. The first was directed to Mr. Rodgers. It had been said by several speakers in the discussion that they thought the results on consistency were no more than figures obtained in a particular *ad hoc* laboratory experiment and did not necessarily apply to normal industrial practice. Mr. Rodgers had not specifically commented from that point of view and it would be of interest if he would comment on the practical significance of the results.

He asked whether the authors could give their views about action which could best arise from their findings. Were they thinking in terms of more training or national standards or some other development?

H. GURVICK (G. D. Peters) said that there was one thing which had been impressed upon him during his time-study training which had not been brought out sufficiently during the meeting. The character of the work within the plant varied from department to department, and one had different time-study personnel in each department. Many of those who had spoken were concerned with mass-production industries, and the circumstances there were rather different from those of small batch work. Within each of the departments one got a different characteristic and a different tempo. He felt that much of the discrepancy which existed, as had been shown, was due to that difference in tempo.

Mr. RODGERS said that *Measurement of Productivity* answered Dr. Andrew. Certainly his work at Acton was an *ad hoc* experiment in a laboratory, but this publication consisted of findings in an industrial situation. They had found exactly the same answer being obtained, or a very similar answer.

Mr. Rodgers referred to investigations carried out as far back as 1933, from which the findings were that only 8 per cent of the observers assessed the standard values of a factory to be just ones; that 92 per cent advanced incorrect values, and that the tendency was to underestimate personal ability. He was not suggesting that the assessments which were right were indeed right and that those which were wrong were indeed wrong. He was only saying that way

back in 1933 people had been getting figures of that description. It was found that only 12½ per cent of the observers showed throughout that they rated too low, and about 80 per cent rated too high. Comparable results were found for different countries and different working situations.

On measurement standards Mr. Rodgers asked whether they had an adequacy of yardsticks for the measurements that they attempted? He felt that the talk of people walking at 3 m.p.h., dealing cards or shovelling, while very useful for illustrating the principles of rating or assessing a performance and perhaps applicable to operations which might be identified with or associated with what they had seen, was inadequate for the variety of operations and the great spread of industries as yardsticks. What they had to look for were many more measuring instruments. The workers in one field under discussion seemed to be content with a universal tool. All the other sciences were constantly devising new measuring instruments and new tools. If they could do that within a firm and then within a particular region and finally on a national basis, they would be coming nearer the mark, but they might still be a long way from what they were really attempting to accomplish.

Mr. Rodgers said that management had to decide what was wanted from time study. Two speakers from industry at the meeting had flatly contradicted one another. One had said that time study was used for standards, and the other had said that it was not. Much of the discussion had suggested that its main purpose was the installation of some form of incentives payment. Possibly time study did in part accomplish whatever it was that management wanted, to management satisfaction and to the satisfaction of their workers. But if the search was for some increase in output at a reduced cost with as good quality or better and with the minimum of discord, were there not other tools which would do the job better?

For example, the snap reading technique, developed long ago by Mr. Tippett who, with his colleagues and others in the textile industry, had used the method with considerable success. But it had been left to the United States to develop and use that technique in fields other than the textile industry, and the speaker suggested that inquiries should be made into its possibilities. It should obviate completely the use of a stop-watch, and he did not think it was necessary even to talk about the assessment of an operator's working performance. One could establish the norm quite satisfactorily. He realized that one had to have a considerable number of units to establish the norm, and, therefore, for a jobbing shop it might be difficult to use such a technique, but it could be used for other

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purposes within the jobbing shop. The spotlight could be turned on to the utilization of the machine tool. Another great advantage of snap reading was that one did not consider the operator and did not focus one's attention on him; rather one focussed one's attention on the utilities which were at his disposal. One inquired into the reasons for the delays or hold-ups, and one got the proportions of them and could take managerial action. They had conducted investigations at the D.S.I.R. in relatively simple circumstances and found that they had been able to increase output by about 60 per cent in one instance (the pouring operation in a foundry), and by considerable percentages in other instances, ranging from retail departmental stores to the handling of incoming timber in furniture factories.

He merely presented that as an additional tool at which they ought all to be looking, not as something which wiped out the very important subject that they had been discussing but as something that might make an extremely useful contribution to the whole subject of productivity, depending on what it was that management wanted. If management wanted to establish bonus schemes and incentive schemes, that was one matter, but if management was looking for output with good human relations and good machines and labour utilization and wanted to put its finger on what was going wrong and causing the difficulty, he strongly recommended the technique of snap reading.

Professor MATTHEW said that an appropriate point on which to wind up was the question asked by Mr. Andrew about what action was required. Mr. Rodgers had touched on some of the things that were required, but he would go back still further. He believed that they should start with the selection of the time-study engineer. People who had an aptitude for that type of work should be selected and should be trained, using a standard method of training, within the company.

The establishment of plant standards was another important aspect. He had already mentioned that it was open to management in any company to devise and use checks right through. The longer the time-study engineer was with a company the more background data he had and the more he got to know about his "patient". He knew the tempo of different operations. He knew a certain amount before he even came to his time study. That was one form of check.

There were other forms of check and control. He had mentioned the use by the chief time-study engineer and the plant manager of a study of distribution of performance or earnings, and even if there was no incentive, there was still a study of performance to be made. There were also controls on consistency which the individual

engineer could apply to his own work. That was one of the most significant things that had come out of the survey which had been conducted at Birmingham.

The current meeting had been pressed for time, and it had not been possible to develop the themes which had been raised. But a paper by Mr. Desmond, published by the Royal Statistical Society, indicated a simple method which could be used by the individual engineer to test his internal evidence and the consistency of his own study. That answered part of the need. If those methods were used, it would be an indication to the engineer that he should re-study the job when there was variability which might not be acceptable to him. The nearer they got to the individual engineer putting controls on his own performances the better control they would have over the situation as a whole. The quality of time study could be raised by such means to a very high level indeed.

He agreed with Mr. Rodgers that they should use other types of checks and controls on time study. Since time study was originally developed they had seen the snap-reading method or the activity ratio method being used widely. It was surprising how many different areas in industry were finding it fruitful to use those methods. He felt that Mr. Rodgers had over-claimed for the method, but it was a useful technique. So were other predetermined movement systems and the M.T.M. work factor method. They could effectively be used as a check on time study.

Those were the lines along which to take action which could have a profound influence on the development of time study because its development for use for management purposes would depend on continued acceptability by both sides of industry.

THE USES AND ABUSES OF ANALOGY

by

VERA DANIEL

IN ATTEMPTING TO APPLY physical principles to human affairs, a very suspect subject, the present author used an analogy as a starting point in an earlier article[1]. Other people have done this before and their results are usually unsound. I shall have to justify my claim that my own approach uses the scientific method.

This paper falls into two parts: the first discusses the use of analogy in science and sociology, the second tries to show that my

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own excursion into sociology constitutes a use and not an abuse of analogy.

The word analogy was originally used by the Greeks to mean a proportion, that is, say, $x : y = a : b$. Later it was extended to cover all sorts of qualitative similarities between two terms. For instance, we may say that someone sleeps like a dormouse. In this wider sense, the term analogy shades off into poetry or libel, and it is difficult to see where to draw the line.

In spite of the wide and vague use of analogies in everyday life, they play a very important part in scientific reasoning. The logic of scientific induction has been treated by a number of philosophers, and owes much to J. S. Mill. The logic of inductive reasoning is much less clear cut than the logic of deductive reasoning because in deduction we make our own rules, while in induction we have to take our data as we find them. Rules can be given for scientific induction, but they are not cut and dried and leave many cases which have to be judged on their merit. Successful induction seems to be a good habit, and when one reads books on logic one is surprised to remember that science does produce some very reliable results.

To discuss inductive reasoning by analogy, I shall give a much quoted example:

Mars is a solar planet,
The earth is a solar planet,
The earth is inhabited,
Mars is inhabited.

Because Mars and Earth have one feature in common, they are supposed to have other features in common, too. As it stands, the argument is very weak, it seems to be jumping to conclusions on the basis of very slender evidence. However, it becomes more plausible if we add that Earth and Mars both have atmospheres, and that their surface temperatures are not too widely different. Even so, it is very dubious, and we do in fact not know whether Mars is inhabited or not, because we have no means of observing life on it directly, and because it is altogether difficult to procure evidence on the subject.

However, we could make quite a good shot at solving the problem even without observing life on Mars directly, if we could get more data on other planetary systems. We should have to observe various planetary systems, with planets that have atmosphere and others that have not, planets that have life and others that are uninhabited, and should have to observe as many other features as possible. If we had enough data, we should probably find that life has certain necessary conditions, and we might also find that in a

certain combination of circumstances life always occurs in a large number of observed cases. If this were so, and if Mars had that combination of circumstances, we should have a good reason for saying that Mars is very probably inhabited. We could not be quite certain, but we should have made a sound argument by induction.

In constructing the argument, we should have used quite a number of analogies, but we should have also done some verifying. We should have said: up to now, features A, B, C, D, always went together; let us look whether they do in the next example that turns up. We should, of course, be able to save a great deal of data-collection if we could make even one controlled experiment where features A, B, C, D, are combined in the laboratory.

In assembling analogies, we usually feel that some are essential and some not. For instance we feel that the fact that Mars is a solar planet is rather irrelevant to the occurrence of life. However, we cannot know *a priori*. If we say that a person sleeps like a dormouse, we do not expect man and dormouse to be similar in other ways too. But that is because we know a good deal about these creatures.

In physics we use observed analogies in the way outlined, but we are also lucky enough to be able to use mathematics. I believe this means that we have access to some rather wonderful analogies. If a physical phenomenon obeys a mathematical equation, then essential features of it are analogous to the relationship between certain abstract symbols, which we can write down. The observed phenomenon resembles closely something extremely simple, with very few attributes. The resemblance is so close that the equations are a kind of working model, from which we can predict features of the real thing which we have never observed.

To arrive at a good theory, a physicist has of course to do a great deal of work. He firstly has to arrive at an hypothesis, as best he may, about the relationships that really matter. For instance, when we formulate Ohm's law we neglect bad contacts, which may be quite a feature of our experiments. However, we find out the conditions in which the experiment should conform to theory, and are not satisfied till we have made an experiment that does.

When theories are checked by experiment, quite a number of likely looking hypotheses are thrown out, and the result is sometimes quite surprising. One particular example throws light on the concept of essential analogy. When we look at ships of different sizes which are exact scale models of one another, we naturally expect them to be alike also in the way they move in the water. However, they do not, as everybody knows: for dynamic similarity they have to be alike in having the same Reynolds number. This means that we

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do not know whether an analogy is essential until we have checked by observation and experiment.

In spite of deceptive analogies, physicists have no fear of the use analogy in general. Any analogy will do if it promises to lead to a theory that can be checked by experiment. For instance, I am told that nuclear physicists work out theories of the nucleus in analogy with the condensation of a drop of liquid. This does not look an essential analogy to the uninitiated, but it is used without fear, because experiment will soon enough show whether it is any use.

As against the high reputation of analogies in physics, their reputation is very low in the social sciences. And in particular, their reputation is low when they are physical analogies. Why is that? The reason will be fairly clear when I give a few examples.

H. C. Carey, in the nineteenth century, stated among others an analogy between gravitation and the concentration of people in towns: all particles of matter attract one another in direct ratio to their mass, and in inverse ratio to their distance. Analogously people attract one another also in direct ratio to their mass, and in inverse ratio to their distance, and so they become condensed into towns.

W. Ostwald, who was a scientist, said around 1910, that from the energistic point of view "culture" is nothing but a transformation of crude energy into useful energy. The greater the coefficient of useful energy obtained in such a transformation, the greater is the progress of culture.

L. Winiarski, at the end of the nineteenth century, tried to base a theory of society on thermodynamics. He said that a social aggregate is nothing but a system of points, i.e. individuals, who are in a perpetual movement of approaching and withdrawing from one another. The primary cause of these movements is attraction and repulsion. These forces tend towards a maximum of pleasure, and Winiarski tried to define some kind of thermodynamic laws whereby these changes can be described. He concluded that in the future society tends towards a state of maximum social entropy, manifested by an equalization of social classes, races, etc.

Still other sociologists, first of whom was, I think, Herbert Spencer, have compared society to a living organism. For instance, Oswald Spengler's book *The Decline of the West*, a best seller between the two wars, is based on the analogy between the ageing of living organisms and the ageing of civilizations. Western civilization had grown old.

Some of the analogies quoted are simply absurd, while others make one feel that there may be something in them. However, none of them can be verified in the scientific sense. In the plausible last

analogy, something complicated, namely human society, is compared with something equally complicated, such as a living organism. We do not know what makes a living organism age and die. Even if we were prepared to accept the analogy between society and organism we could learn very little from it. As for the more crude, physical analogies, the first one, that concerning mutual attraction in the ratio of mass, seems to be demonstrably untrue. In the analogy concerning crude and useful energy, it would seem impossible to define the terms. The thermodynamic case would involve the author into calculations of fantastic difficulty, if he were to take it seriously, and it is also a comparison of the complicated with the complicated.

In considering the examples quoted two questions present themselves. First, in what conditions can we hope to apply scientific reasoning to social phenomena, if at all? Second, why are otherwise intelligent people satisfied with shockingly slovenly reasoning when they are concerned with the social sciences?

I shall take the second question first. There is a fairly obvious reason in that we all, scientists included, are involved emotionally in human relationships. When reasoning about human affairs, we try to find reasons for our preconceived ideas. However, there are other reasons also for the misuses of analogy in the social sciences. The misuse of analogies creeps in also in science in fields where experimental check is not easy. For instance, in the theory of lightning, which is a phenomenon inaccessible to laboratory experiments.

Lightning is in many ways analogous to the discharge of a condenser, where the clouds signify one electrode, the earth or another cloud the other electrode. For a discharge in air between conducting spheres, observation shows that the path of the discharge always forks away from the positive sphere. In lightning, observation of earth strokes shows that lightning always forks away from the cloud. Hence, it was concluded that cloud must be positive in relation to earth. In fact, it is the other way round: the cloud is negative. However, I am told that the wrong conclusion from analogy persisted for quite a long time. In the end, the conflict was resolved by laboratory experiments which proved that in certain conditions a spark will fork away from the negative electrode.

Another example, rather more controversial, is the use of the Maxwell-Wagner mechanism for the explanation of dielectric behaviour. Here we have a quite valid mathematical analogy between the behaviour of a piece of dielectric material and an electrical circuit, or a combination of circuits, consisting of capacitors and resisters. For a given electrical behaviour of a piece of material we can always devise a circuit which would give a similar response. However, there

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is no clear correspondence between the features of the circuit and the features of a complicated piece of material, say a polymer whose texture is not clearly understood. By finding a circuit that gives the same response as the material, we have proved only that the observed data might be explained by some mechanism such as that postulated. We have proved that the behaviour is not inexplicable, but we have not explained it. It could only be explained in terms of the actual structure of the material. Nevertheless, the fact that here we have a circuit diagram representing something we have measured, has a kind of hypnotic effect and we accept it as an explanation.

One of the worst temptations of analogy lies in the fact that it embodies an important feature of explanations in general. Ernest Mach said that to explain means to reduce ideas we are not used to think to ideas we are used to think. An analogy always does that even if it does not provide an explanation. It is psychologically satisfying.

Reasoning by analogy, without scientific checking, is in fact a very natural process with all of us. Children do it a great deal. For instance, I know a story of a little boy of four in Hungary whose parents lived in the country and had a black horse and a white cow. The little boy had every morning black coffee with white milk for his breakfast. And so he formed the hypothesis: our cow gives the milk, and our horse gives the coffee. It is rather a pleasing analogy with its kind of symmetry, rather reminiscent of Maxwell's equations. Unfortunately, it does not work.

Analogies seem to play quite a part in primitive religion, too, in sympathetic magic: if a witch makes a clay image of a person and sticks a pin in it she hopes that thereby the person will die, by analogy. Sometimes these analogies are very odd. For instance, I think we all know the child's game where you walk along a kerb without stepping on a join. Often a child connects this with something else, for instance: if I do not step on a join, I shall not be asked the seven times table. Some sociologists think that there exists an instinct for making combinations, to the effect that two unconnected things *must* be connected. This is manifested, for instance, by connections that are postulated between the courses of the stars and human fate. Analogies satisfy this instinct.

All this leads to the conclusion that normal people are often tempted to use analogies rashly. If they are scientists, and if they deal with scientific subjects, they resist this temptation. This is partly due to their training: everyone doing research has probably jumped to conclusions some time and found that he had to retract his steps and do a lot of extra work in consequence. Besides scientists, as scientists,

have their reputations to think of, and jumping to conclusions, at any rate in print, is very bad for one's reputation. However, in the social sciences these wholesome factors operate much less, and so it happens that even scientists may greatly misuse analogies where they venture into social science.

This brings us to the other more important question: if scientists venture into the field of social science, using analogies or otherwise, can they preserve their mental discipline, and how can they do this? I do not think this can be answered in general. Human society is a phenomenon in which innumerable variables operate, and it may not be possible to isolate useful relationships between any of them. However, if a scientist ventures into social science, other than as a technician helping social scientists, then he presumably does so because he has a hypothesis of sorts. In that case I think one can define conditions which his argument must fulfil if it is to be scientific:

(1) The hypothesis must concern variables that can be clearly defined.

(2) These variables will be only a few of the many operating. The scientist must define in what conditions he expects to be able to neglect the variables he does not consider.

(3) The variables in question should be measurable, or at any rate capable of being estimated.

These conditions do not of course mean that sociological arguments not fulfilling these conditions are necessarily valueless. I only claim that they cannot claim to follow the scientific method. They belong to art rather than science.

Having stated some rather difficult conditions which a scientific hypothesis in sociology would have to fulfil, I shall have to prove that my own excursion into sociology does fulfil these conditions.

The argument I am proposing starts out from the observation of an analogy. During the war, I spent three weeks in one of the "Lend a Hand on the Land" camps. On the first Monday, when about half of the members of the camp were new, some twenty or thirty girls were put into a lorry and deposited on a hilltop, by the side of a large field, which had been experimentally sown with turnips and kale, with the idea that the turnips could be pulled out if the kale grew well. We were told to pull the turnips out, and left to ourselves. For an hour or two all was well, everybody working fairly diligently. However, after that a few girls seemed to have had enough, and sat in the hedge eating sandwiches and looking at the view. The rest disapproved, since we had not been working very long. However,

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when a few more stopped working the mood changed; most people felt that it was unfair that they should be working when others took it easy. Now the party broke up with a rush, and soon all except some five stalwarts were sitting in the hedge. The number working as a function of time, looked something like the curve shown in figure 1.

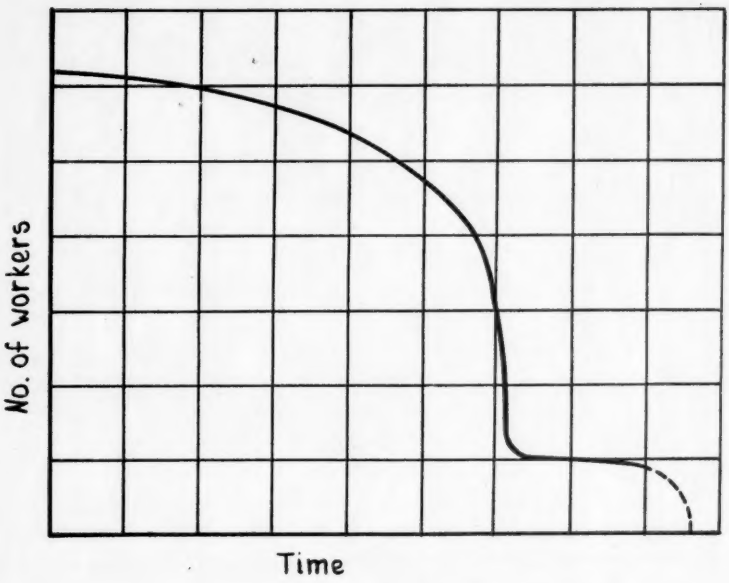

FIGURE 1.

To a physicist working on the properties of materials this observation suggests rather forcibly an analogy with magnetism. The elementary magnets in the theory of magnetism are aligned by means of the internal field, which is in turn due to their mutual interaction. An increase in temperature disturbs the alignment, in other words, the order of the arrangement, and in doing so weakens the internal field which enforces the alignment. This means that the response of the alignment to a rise in temperature is strongly non linear. The degree of order as a function of temperature always follows a curve of the kind shown in figure 2. The order breaks down more or less suddenly at a transition point or "Curie point".

In order to make quite clear what is happening, I refer to model of a magnetic material. It consists of magnet needles which pivot on pins stuck into a board. The needles may point either left

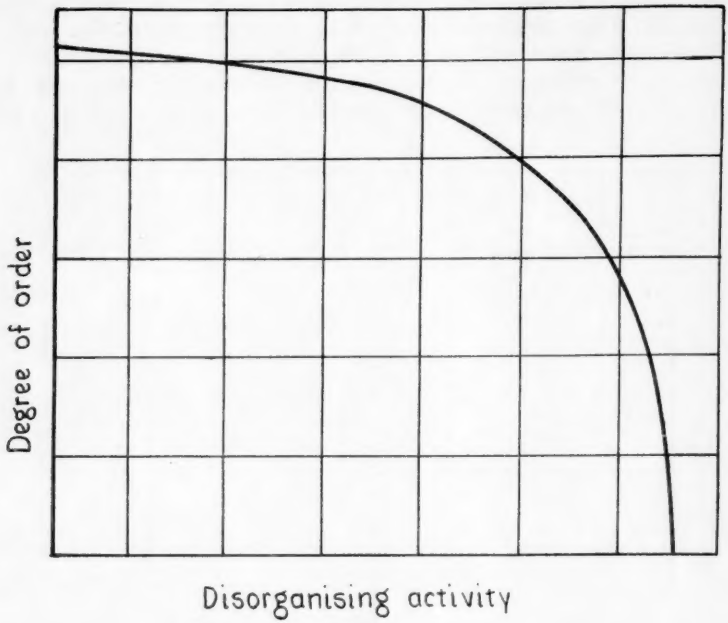

FIGURE 2.

or right. When they are not disturbed, the attraction between north and south poles and repulsion between similar poles makes for an ordered arrangement. When the needles are agitated by temperature motion—say by some cunning random sand blast—some needles are turned round out of their most stable position. As long as only a few needles do this, each is opposed by a strong internal field due to the interaction of all its neighbours. Hence the temperature motion is not able to disorder the pattern much. However, once the disorder is sufficiently large for a disordered needle to have disordered neighbours, it is no longer strongly compelled

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to return to the old pattern. Now a slight further increase of temperature is enough to destroy the order altogether.

In the human case, we have also a pattern of order: all should to their work—and an internal field which acts upon each member. This "internal field" is the public opinion which disapproves of the defaulter. Against the order there operates a disorganizing influence, namely the tiredness or boredom of the individual. While only a few are tired or bored, public opinion is strong, because it is due to the mutual influence of the participants: "why should a few lazy ones shirk when we all are working?" However, as the disorganizing influence becomes stronger, public opinion weakens, and order breaks down.

The analogy I have outlined between a breakdown of morale and a Curie catastrophe in a magnet is striking, but by itself it does not mean very much. If one looked round sufficiently, one could probably find many amusing analogies between human behaviour and some phenomenon in physics or chemistry. An analogy of this kind can only be useful if it leads the way to the discovery of some concepts on variables which are useful for scientific analysis of human affairs. My claim is that this analogy can be developed to that purpose.

The order/disorder transition observed in the model of the magnetic needles is a special case of a co-operative phenomenon. Such phenomena are fundamental for the structure of solids (and to some extent also of liquids). The particles which make up a solid exert forces on one another, and they have temperature motion. At absolute zero, the forces of the particles on one another are almost always satisfied best within an ordered pattern, i.e., if the material is crystalline. Even for a polymer, with very many different kinds of particles, the ideal structure at absolute zero would be more or less crystalline, though not quite as regular in its repeat for a pure substance.

Whenever a regular pattern is the best way to satisfy the forces between particles, the order is maintained by mutual interaction. That is, each particle is constrained to its position by its neighbours. If it strays from its position under the influence of temperature motion, it is opposed by the combined influence of its nearest neighbours, who in turn are backed by the more distant ones. This has the effect that order, as a function of temperature behaves always in the way shown by the figure: it breaks down more or less suddenly. There are complications, in that often one pattern of order changes into some other pattern of order, and the character of a Curie point is often obscured because changes have no time to go to completion.

Nevertheless there remains the general conclusion that patterns of order break down with rising temperature so that there is a sharp transition point.

In physics there exists a number of different approaches, working with different concepts and variables, to the treatment of crystal structures and their changes with temperature. Among these, there is one which is not very much used because it is not very suitable for rigorous quantitative treatment. This is the Curie-Weiss theory of magnetism, and its counterpart is the theory of alloys, the Bragg-Williams theory. These theories use as variables: degree of order, internal field or organizing energy, and temperature. It is these variables which I have tried to demonstrate on the model of the magnet needles. My claim is that they may be used with advantage in the theory of social groups.

To recapitulate: if we have an assembly of units who mutually influence one another in some way, there exists usually some ordered pattern which satisfies the interaction of the units. A pattern of this kind is held together by the influence of neighbours on one another. As against the order of the pattern there operates the irregular temperature motion. With rising temperature the order breaks down more or less suddenly, at a transition point. This description is valid for a great many types of units and of patterns, and my hypothesis is that it may apply to certain human assemblies also.

In human society, we have individuals who influence one another mutually, and who are at the same time spontaneously active. People are of course much more complex than any unit in a crystal, and no two people are alike. Nevertheless, all people have certain things in common with each other, and some people are rather like some other people. This means that it seems reasonable to suppose that the mutual influence of people on one another can be satisfied by some pattern of order. If this happens, we may expect a relationship between degree of order, public opinion, and spontaneous activity, rather like in physics.

Up to this point the argument is no more than an hypothesis, based on more or less plausible reasoning. By my own prescription, I am now under the obligation to define my variables clearly in terms of the human society, to postulate conditions when the variables should be the only important ones, and to suggest experiments whereby the prediction of the theory can be checked.

Before doing this, I should like to show that it is worth while to apply the hypothesis to human society. My argument only states that it looks as if people might form some ordered patterns. There would be little point in going on if phenomena resembling such

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patterns was not in fact observed. In showing this, I am a little handicapped because I am speaking to an English audience; in this country, ordered patterns are relatively little prominent. However, anthropologists tell us of many distinct "patterns of behaviour" in primitive society, and we all probably have participated at times in groups with distinct patterns. For instance, school forms have sometimes very distinct patterns of behaviour. Also, there is the well-known paradox of national character whereby people of different nations, *en masse*, behave very differently, while they are much more similar as individuals.

I shall now attempt to define my variables. To the pattern of order of physics corresponds, in human society, a pattern of behaviour. This can be clearly defined in the case of a primitive tribe with rigid customs, but is not always as clear. To the degree of order corresponds the proportion of people conforming to the pattern, and to the internal field corresponds the strength of public opinion. So far, the variables are as clear cut and simple as one may hope for in social science. The variable that is most difficult to define is the temperature, or disorganizing activity. It is not possible here to discuss this at all fully, and I shall only point out that it is due to the needs and desires of members of a group which find no fulfilment in the group activities. In some cases, the disorganizing activity can be influenced by some physical factor, as in the example of the people weeding in the field; here time is likely to increase the disorganizing activity.

The next stage is to define conditions when we may expect the variables in question to be the most important ones for the description of a social phenomenon. To this, the answer is: they should apply to groups, the members of which all subscribe to, or are subject to a code which implies a pattern of behaviour. They are likely to operate most clearly in isolated groups of this kind, the ideal being, say, a tribe living on a small island. Whether or not a given social group may be treated as an isolated single co-operating group would clearly have to be decided on the merits of the case.

The next stage is the design of experiments. I have already suggested one experiment, namely that of the group of new land girls in the field. Here we can measure degree of order by the output, or the number working, and we may assume that disorganizing activity is more or less proportional to time. A more spectacular example of this kind would be the morale of bomber crews during a war, as a function of the number of flights a squadron has made. Here it would be necessary to measure morale in some way, but in dramatic conditions that may not be too difficult. I believe work of this kind

has been done during the war, but I have not yet been able to learn the results.

It has been shown that the hypothesis made so far is capable of verification. It has not been verified yet by controlled experiments, and of course it may prove to be mistaken. However, on the basis of unchecked observation, such as that mentioned, I think that it is probably correct. If it could be proved, I think the argument could be put to immediate use, if only in specially favourable conditions. The recent strikes have made me think of two possible applications of a quite simple kind.

If a strike breaks out in an establishment, for local reasons, and if the workers form a single co-operating group, we may expect the breakdown of morale to be co-operative, culminating in the strike as its Curie point catastrophe. If that is so, morale should become worse and worse as the strike condition is approached. If morale could be measured—and the Acton Trust has shown that absenteeism is to some extent a measure for morale[2]—then we should expect morale to drop catastrophically as the strike is approached. In other words, absenteeism as a function of time, just before the outbreak of a strike, should be different in establishments where the strike is supported by a mounting resentment from that in establishments where workers are called out more or less against their will.

The last example should, of course, be deduced also directly from psychological considerations. The next example is perhaps slightly more subtle. I happened to hear that one particular bus garage voted to go back to work, with a majority of one vote. This is very odd if the busmen formed a single co-operating group because in the order/disorder curve a proportion of 50 per cent. in favour, 50 per cent. against is almost certainly in the steep part of the curve, so that it may be expected to be found very rarely. On the other hand, the busmen may consist of two groups, evenly matched, in which case the vote recorded seems quite natural. Indeed my informant said that the single men wanted to strike but the married men felt they could not afford it.

The argument advanced so far has only rather limited significance, because breakdowns of morale in single co-operating groups are not very common events. It is therefore very tempting to carry the argument further, beyond the simple experimental conditions in which one may hope to verify it. One may apply the variable defined in more complicated conditions, and one may use the model that served so far to predict further relationships. I have done both, with some fear and trepidation, and in one case at any rate the method seems to work remarkably well.

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The model of the magnet needles is a model of a ferromagnetic material. That is, when we apply an external field at some temperature near the Curie point, we get a response very much greater than we should if the magnet needles were independent of one another. This is so because the mutual interaction of the needles tends to make them parallel to one another, independently of whether all point right or all point left. In consequence, an applied field can turn the assembly as a whole, instead of turning each needle separately. This model can explain the fickleness of crowds. If a number of people are excited and feel that they must do something, and do it together, but do not know what exactly they should do, then an external influence can sway them much more easily than if each individual had to be swayed separately.

I am not sure whether this application of the model is a legitimate use of analogy. It has certainly to be treated with caution and one cannot say that the fickleness of crowds must be due to this cause. However, I think the argument shows that the fickleness of crowds can be explained without having recourse to human characteristics, such as the hypnotic effect of the leader. This is a definite result, even though it may not be safe to rely on the model beyond it.

If the hypothesis advanced were to be proved in the conditions where it should be verifiable, there would be a strong temptation to extend the use of the concepts and variables introduced also to conditions too complex for a scientific verification. For instance, it would be interesting to discuss the significance of democracy and totalitarianism. Such an analysis does seem to work in that it fits in with qualitative observations, and in that its predictions lead to conclusions that seem to agree with what a Liberal likes to hear. They are that we want in essentials unity, in non-essentials diversity, and in all things charity. However, one can prove almost anything by arbitrary examples chosen from history, and these conclusions can hardly claim to have been arrived at by an application of the scientific method.

In conclusion, I think it is fair to say that the use of analogy is a legitimate and important part of scientific induction. There are pitfalls in it because analogies tend to make us jump to conclusions, but in natural science people rarely fall into this error, or if they do, they are soon pulled up by their co-workers. In social science, on the other hand, analogies seem to lead to trouble very often, so much so that the use of analogy has become suspect. I think that is not the fault of analogy, but the fault of social science. The difficulty in social sciences is that they deal with phenomena that involve innumerable variables and that hypotheses can only rarely be

verified. This means that research workers need very finely balanced judgment and that they have no chance to be taught this judgment by making their own mistakes.

In the circumstances, it seems reasonable to give up the social sciences as a bad job and be glad that we are scientists. However, in this period of history it would be very important to have a social science that works—as has been said for instance by Professor Adrian in his presidential address to the British Association. Because of this, I think one ought to try to bring scientific ideas into social science, even though it is difficult.

I think the main fault of the earlier attempts to apply scientific hypotheses to human society was their arrogance. The theorists used to say: human society is nothing but—and then came some statement about what it was supposed to be. I think one must always be aware that human society is everything else as well. If we do make hypotheses, we must always be aware that they can be valid only up to a point. In checking them, we must use all we have learned in science, and all the knowledge of human nature we have, and not forget that our knowledge of human nature is always incomplete.

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[2] *Size and Morale.* Acton Society Trust, 1953, London.

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* Paper given to the Operational Research Society, 19 November, 1954.

ABSTRACTS AND REVIEWS

Queuing for Medical Care.

NORMAN T. J. BAILEY (Nuffield Trust).

Applied Statistics, November, 1954; **3** (3), 137–145.

The article indicates how the statistical theory of queues can be applied to calculate the number of beds in a hospital, the number and length of clinical sessions, and the appointment system, necessary to provide services with a defined amount of waiting on the part of patients.

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The Use of Estimates of Absence-Proneness for Guiding Executive Action.

A. G. ARBOUS and H. S. SICHEL (N.I.P.R., South African C.S.I.R.).

Applied Statistics, *November*, 1954, **3** (3), 159–173.

A way is suggested of using past records of absenteeism to select members of a labour force who are suitable for special treatment because they are likely to be absence-offenders in the future.

Operational Research.

TATSUO KAWATA (Tokyo Institute of Technology).

Statistical Quality Control, *June–October*, 1953; **4** (6–10). (*5 articles in Japanese.*)

A general description of operational research developing the theme that problems which seem superficially to have no resemblance to each other become essentially the same type of problem when formulated mathematically. The formulation of mathematical theories in relation to their various applications is regarded as one of the most important roles of operational research.

Introduction to Operational Research.

C. SALZMANN.

Ingénieurs, *March*, 1954; **49**, 22–28. (*In French.*)

A general description of operational research with war-time examples.

An Introduction to Linear Programming.

JOSE CASTAÑEDA.

Revista de Ciencia Aplicada. May and July, 1954; **8** (38), 203–217 and (39), 321–330. (*In Spanish.*)

The problems of maximum and minimum conditions arising in the quantitative study of economics are indicated together with their resolution in simple cases by geometry and by the Dantzig method, with a numerical example.

Alternative approaches to problems involving maximum and minimum conditions are explained and the so-called "Hitchcock problem" is considered.

Bulletin du Seminaire de Recherche Operationnelle.

Institut de Statistique de l'Universite de Paris, January 1955. (*In French.*)

An account is given of an operational research seminar held at the Statistical Institute of the University of Paris.

A first paper by G. KREWERAS, *Equations of Stock Control*, discusses purchasing policy in a manufacturing industry. The problem is to decide whether to buy raw materials as they are required, regardless of price fluctuations, or to buy in large quantities when prices are low, and put to stock. The author shows how the problem can be formulated mathematically and gives a worked-out example which shows that a purchasing policy based on what appears to be sound commercial practice can, in fact, be uneconomical.

The second paper by E. MALINVAUD, *Introduction to the Study of Linear Programming*, gives a brief description of the aims and method of linear programming and shows how this technique can be used to solve production and distribution problems. These applications are suggested: firstly, a transport problem; secondly, a diet problem; and thirdly, a high-level policy issue, namely to decide the best way in which to invest the French electricity authority's capital to give the best possible returns—the choice being between building steam power stations, one of two types of low-head hydro-electric power stations and high-head power stations.

Organizing Work for Stevedores.

A. C. BOS.

Tydschrift voor Efficientie en Documentatie, 1954; **24**, 262–265. (*In Dutch.*)

The author describes a system of organizing work for stevedores used by the K.N.S.M. (Royal Netherlands Steam Ship Company). Compared with previous methods the following advantages are claimed: (a) concentration of a large number of individuals in a restricted area is avoided (important at a time of industrial unrest); (b) the assignment of work is easily understood by all concerned; (c) work can begin at scheduled time; (d) reporting at the wrong place (deliberately or otherwise) is impossible; (e) accurate time check is possible; (f) the foremen no longer need register workmen's names; (g) at any moment the number of individuals at work is exactly known; (h) the system provides a sound basis for the administration of wages and merit allowances.

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THE HANDLING OF UNCONTROLLED DATA

by

H. G. JONES AND G. H. JOWETT

UNCONTROLLED DATA CAN ARISE from two distinct sources, namely, either from the routine logging of observations required for the day-to-day productive running of the plant, or from special observations of particular components during the normal running of the plant. The second do not differ from the first, except in so far as they are not normally available; uncontrolled data are regarded by the writers as any in which the variables may have approximately continuous values in contra-distinction to controlled tests in which one or more factors are held at distinct levels. In addition, controlled tests frequently involve the disposition of the test in a predetermined manner with respect to time.

An analysis of uncontrolled data might alternatively have been defined by describing it as the opposite of a planned experiment. It is the general desire to be associated with planned or factorial experiments, both because the analysis is simpler, and because one is more likely to get a positive answer for a given amount of work. But it cannot be denied, however much one regrets it, that much of the analytical work in heavy industry arises from uncontrolled tests. By illustration from actual examples met by one of the authors and his colleagues in a modern, fully integrated steelworks, it is

hoped to demonstrate in the first half of this paper that there is considerable value in examining uncontrolled data, however unfashionable that might be. A review of the pitfalls to be avoided in such work will then be given in the second half by the other author.

Many people have been examining this form of data for years, and at the risk of some glimpses of the obvious it is proposed to enumerate below six methods by which a start can be made. Analysis may be (a) by classification, (b) by spatial pattern, (c) for cycles, (d) for trends, (e) to eliminate trends, or (f) for relationships.

While the first three items of this list can be treated in a fairly elementary way, it is emphasized that in many phases of industrial life, one is only concerned with finding a solution sufficiently exact for effective action to be proposed, and thus many of the short term problems which come along in a works can be solved by the simpler techniques. It is suggested that the more sophisticated techniques should be reserved for those problems which cannot be solved otherwise.

Analysis by Classification

Analysis by classification is one of the oldest and still a powerful way of examining large masses of data, whether the classification is done by hand sorting or by elaborate mechanical devices. The first example concerns information obtained from the very adequate records, kept by the Works Traffic Department, of the turn-round times and delays experienced by lorries exporting part of the finished product. At present, more than 1,350 lorries per week enter the works for this purpose and there is clearly scope for a fairly considerable congestion to occur in the service bays.

The times recorded over a representative sample period of three weeks were examined on frequency diagrams, first of all for uniformity of working around the clock, and then for each of the operations such as reporting on arrival, weighing empty, travelling to bay, loading, re-weighing, collecting delivery note and also by internal district within the works. Very briefly, it was discovered that the expected distributions were in fact found for the components and districts with theexception of one bay for which the total turnround times extended in a tail some $3\frac{1}{2}$ hours longer than that of the other bays. The management decided that all supplies to this bay should be stopped for 24 hours, and during this opportunity the bay was reorganized in such a way as to eliminate the congestion causing the tail.

It is a common feature of action arising out of classification that the unusual can be pin-pointed from either a skew distribution or a tail, and ameliorative remedies applied.

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Precisely the same set of conditions pertain in the next illustration, that of the despatch of wagons from the works. This work was part of a fact-finding investigation in which it was desired to find out whether the cause of delay was in the works or on British Railways. One operation (that of obtaining the assignment note) was carried out reasonably well but other operations (such as labelling the wagons) showed unnecessarily long tails to the frequency distribution, times in excess of 48 hours being not uncommon. This example is quoted, because in spite of being completely parallel with the lorry illustration, in this case the information was not available in the form of routine records. Observers were required to march up and down the sidings day and night, counting and observing wagons, tracking "official documents", and so on. Even though the times of observation were randomized to prevent synchronization with the movements of the traffic operators, it is considered that this is still uncontrolled data.

Analysis by Spatial Pattern

Pictorial representation in the form of flow patterns has long been recognized as a familiar method of examining the movement of raw materials, and for representing heat losses to mention but two examples from many. There is a well known technique in time study, a string diagram, for obtaining patterns of movement which might not be so familiar to workers in the operational research field. In this technique, a plan of the shop is laid out on a board and panel pins nailed into the plan at strategic positions. The movements are then recorded by winding thread from pin to pin. This method is a powerful device for translating written records to a plan, but is even more useful on the shop floor where crane movements, for example, can be recorded pictorially at the same time as the observations are made.

In a melting shop one row of pins might represent the furnaces fed by charging machines which travel between the furnaces and the stand of scrap boxes shown by a second row of pins. Other pins might represent the raw materials held in wagons or lorries; cranes are employed to handle the various materials from the wagons to the scrap boxes.

The problem is to examine the crane and charger utilization, to look for excessive long travelling represented by long lines on the string diagram, and for crane or charger interference represented by crossing patterns.

A simple refinement introduced by one of the authors is that of using different colour threads for different hours in the shift, so that

the changing pattern of the shop can be recognized. For the purpose of the records the boards are finally photographed.

It is surprising how much information can be unscrambled by this very simple technique, whether it is applied to crane movements, lorry movements or raw material flow. In the example quoted the re-arrangement of the wagons in a shunt enabled an appreciable amount of long travelling to be cut out. An extension of one rail track improved conditions adjacent to one furnace, while the provision of a second door for lorries reduced both the long travelling and interference.

Analysis for Cycles

Recurring events are frequently of interest. Amongst other problems met is that of derailments in which it was found that there is, for the last three years, a recurring pattern for the four quarters of the year, the derailments being highest in the spring quarter of each year. The reason for this peculiar result, since one would expect the winter and spring quarters to be very similar, may lie in psychological and physiological reasons arising at the end of the winter. Differences from a yearly moving average show up the effect of the cycle. The cycle was sought in this case as a clue to the cause. Had winter and spring both contained high derailments, one would have thought in terms of lighting or other physical causes. As it is, one starts thinking in terms of the vitamin tablets, proposed by one firm to counteract winter conditions, as well as of the already generous protection clothing provided.

Analysis for Trends

It is quite common to plot a variable against a continuous time scale with the intention of emphasizing departures from the normal control limits. Reference in this connection will be made to a simple trend chart which is only of other than local interest because it has been applied to a group of ships, each of which is generally regarded as being unique, and therefore, at first sight, not an obvious application for a control chart.

Frequency histograms of the rates of discharge in tons per crane hour were made for all ships visiting the works' wharf in 1952. The remarkable observation was made that in spite of several major differences in design, in size and in the density and physical characteristics of the ore carried, if the ships were divided into two classes only, the good and the bad, symmetrical distributions of discharge rates were obtained. 95 per cent limits were obtained from these distributions and two control charts initiated. For the

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early part of 1953 it was confirmed that the current discharge rates fell within these limits; accordingly a master board was erected on the wharf where the discharge rates for different ships are shown clearly for all and sundry to see, but instead of 95 per cent limits a red line was drawn (between the limits) to indicate that the results were expected to be above this line and an optimum target line was also established which was well above the then existing figures.

Now experience has shown that with the co-operation of the management and the men, and the introduction of new methods and machinery, the discharge rates have risen steadily. It is believed that the wall chart has helped to confirm the pride of the skilled operators in the job they are doing and certainly indicates at a glance to the management the previous history for a year.

In this case the chart was installed deliberately to influence the data; it may be that unconsciously this happens on other occasions and that by bringing an investigating team to bear, either the morale or sometimes the supervision in a department has improved. Although the picture might not be scientifically exact of conditions before the investigation, the investigation in such a case pays handsome dividends as far as the firm is concerned. In fact, during a previous employment one of the writers of this paper indulged in this practice at roughly three-monthly intervals in one annealing problem, with most salutary results.

In passing it might be observed that non-linear scales can sometimes help to clarify lessons not at all obvious on a linear scale.

Analysis to Eliminate Trends

In any works operating furnaces, whether on a continuous or a batch basis, a considerable amount of routine data is collected for years on end. Many attempts have been made to examine this data for cause and effect. The path is fraught with difficulties because in many cases it is not at all certain which are causes and which effects. Moreover attempts to extract principles from particular cases have led to conflicting results.

The practice has grown up of taking one arbitrary factor—usually a measure of the rate of working of the furnace—as dependent variable, and to account for variations in this factor by means of a multiple regression involving as many of the other factors as are found to be significant.

All the operating data for a blast furnace for four years has been examined in this way. If, in addition, the weekly coke consumption per ton of iron produced is plotted against the weekly values of a certain factor (actually the basicity ratio of the slag) for

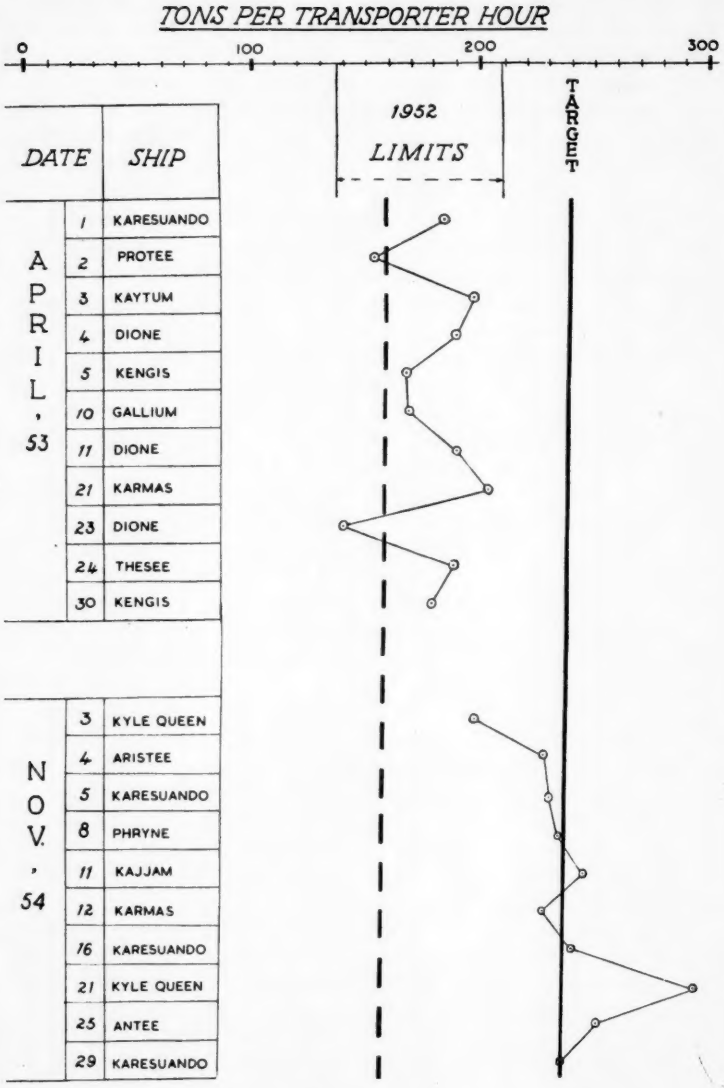

FIGURE 1.—*Trends in ship discharge.*

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the 210 weeks of the study, it appears that the two are fairly strongly correlated. Now four years is a long enough period for time trends to occur in most processes and certainly in a blast furnace which is traditionally regarded as a temperamental piece of plant. A superficial inspection of the data indicated that all the high basicity values occurred at the end of the four years. Then is there a true correlation between coke rate and basicity? Or is this mainly a time factor?

To examine these questions, the data was divided into six weekly periods and it was first demonstrated that this period is short enough for the values to be random and therefore free from trends. Changes in coke rate within the six-weekly period were compared with changes in basicity in the same period. For convenience, the changes were measured from the mean values in the

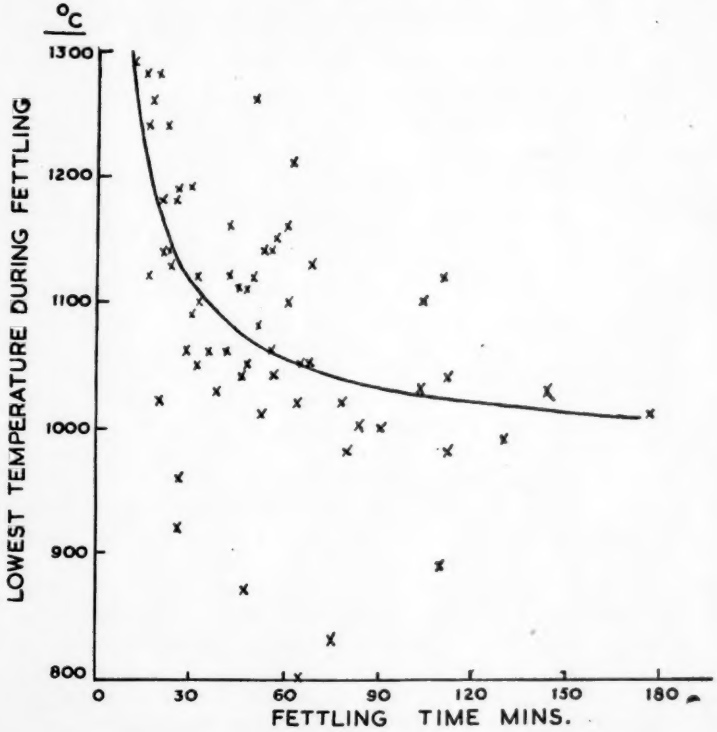

FIGURE 2.—*Effect of fettling on temperature of furnace after fettling.*

six-weekly period. When the changes are plotted for the data for four years, the apparent correlation has disappeared. It is believed that a trend was present, for reasons which are still unknown, and can give a spurious correlation.

One point of interest is that the regression co-efficients have quite different numerical values if the six-weekly difference is employed from those obtained by using the straight data. This has been assumed to be due to unknown factors causing trends. These differences could also appear if the relationships are non-linear at the same time as the observations are not uniformly distributed throughout the range. Without having done considerably more work on a number of furnaces, it would be idle to claim a wide generality for the results, but it is clearly recognized that a method involving differences has decided advantages in evaluating furnace current performance if, as is very likely, the influence of some at least of the factors is curvilinear.

Analysis for Relationships

The obvious method of looking for a simple relationship is to plot a scatter diagram and then to fit a line if such exists. This assumes no *a priori* knowledge of the process.

In a few specialized cases it may be a help to assume a theoretical knowledge of the process and then to fit a curve of the theoretical shape to a mass of data. For example, a curve

$$(\text{Temperature})^3 \propto (\text{Time})^{-1}$$

which is derived from Stefan's law of cooling, has been fitted for the fall in temperature when the fuel was cut off a furnace due to fettling or any other reason. In this case, the spread in results was somewhat large, but the best cubic seemed a reasonable approximation to the truth. The implication of this curve is that the loss in heat is most marked in the first hour, and that fuel off for short periods is just as vital as cases when the fuel is off for longer times.

More commonly, one is asked to find from a mass of data the relative influence of the various factors on the dependent variable. Such a problem was concerned with defects on the edges of steel sheets or coils, which, while they would not reach the customer, were an inconvenience in production and, moreover, caused a loss in yield.

The problem was first simplified by restricting the examination to one of three melting shops, by examining one type of steel only, and by considering steel from one particular size and type of mould. After this simplification, the Metallurgical Department cited some 28 factors suspected of having an influence on this defect. Now it seems rather a lot of work to introduce 28 variables into a multiple

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regression analysis. Accordingly the first stage was to plot scatter diagrams of each of the 28 variables separately against the defect. Ten of the graphs showed a random scatter and so these variables were dismissed from the analysis. There was at least some suggestion in the other 18 that there might be some effect, so the significance of each variable separately on the defect was examined by means of two multiple regressions; the factors in these regressions were selected so that the variables in one regression were not believed to influence the other group. Those factors with a significance of less than 1 in 20 were next discarded; this left eight factors upon which a final multiple regression was carried out.

In passing it might be indicated that this analysis assigned a numerical weight to three factors of a significance higher than 1 in 1,000, one of these factors being under the control of the shop management. Action was taken on this variable and benefits arose, but, as is so often the case, yet another factor not in the original 28 factors—namely a particular practice within the shop—was radically altered about the same time and the problem disappeared by virtue of this action.

Difficulties and Pitfalls

So far, this paper has listed the broad techniques which are needed in the analysis of uncontrolled data, and has shown what can be done with them by illustrations drawn from the authors' experience. If we intend to apply these techniques to such problems, there are certain difficulties and pitfalls over which we must take care, and it is with the more fundamental of these that the remainder of the paper is concerned.

Analysis by Classification

The uncontrolled data in these problems usually form an experience spread over a period of time, and it is from this that the main difficulties arise. In the applications previously described, experiences of turn-round times extending over a period of time were condensed into static pictures (namely histograms) which were then compared as to shape.

For the comparison to be sound, the situation must be static, and the experience representative. This is achieved if variation in, say, turn-round times varies randomly with time (an uncommon phenomenon); or, when the variable is not random but ebbs and flows with time in the manner characteristic of industrial time series, if the experience has been long enough for it to have varied over its whole range often enough to give a clear picture of the

nature of its oscillations. Otherwise a homogeneous-looking histogram may just mean that over the period in question the situation was at an ebb (with the flow to come later), or a humped tail merely the consequence of, say, one rather violent ebb during the experience.

A second difficulty is that there may be unnoticed differential weightings between two classes which bias the comparison, such as different proportions of two kinds of lorries; this ties up with the third difficulty, namely that there may be differences between classes other than the obvious ones associated with the basis for classification.

Since with this simple kind of analysis we are usually content to draw conclusions from a visual comparison, significance being assessed by intuitive judgment, the best precaution we can take is to criticize our classes from these points of view.

Analysis for Cycles

Provided that we are looking for cycles of known period, the estimation of the means which form the phase components is a fairly simple matter. As a rule, however, seasonal cycles have simple patterns, and since there is only a small number of really distinct types, it is very unsafe to infer a causal relationship between two cycles merely from a similarity in shape. In correlating two time series with strongly marked cycles, the precision of the correlation does not increase much with the number of points, since each successive complete period tells the same story all over again; one might just as well correlate merely the phase averages.

Analysis for Trends

There is little that can usefully be said about analysis for trend as used in the illustration quoted, because there accuracy of fit is not critical. Some remarks about fitting trends to industrial series in general, however, may not be out of place.

The modern tendency is to get away from the idea of trend plus random variation as a description of time series, though many methods of analysis are based on this structure for convenience, or as a crude approximation. Trend is here an imprecise term, relative and not absolute; there are relatively quick and relatively slow movements in most time series, and often no clear dividing line between them. What would pass for a trend in a short section of series might well be called part of an oscillation in a long stretch. On the whole, moving average methods seem better than polynomial methods for fitting trends to time series, because in using them one

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explicitly recognizes the arbitrariness of the whole procedure, and has to decide just what aspect of the variation is to be regarded as trend for the purpose in hand, instead of striving to attain an idealistic degree of randomness in the unexplained variation by forcing recalcitrant polynomials of ever increasing degrees of complexity to follow ephemeral undulations of no particular practical relevance. In using a simple moving average to fit trends, they must be approximately linear over the length of your average if it is not to "cut the corners", and such an average will tend to iron out any undulations of length comparable to it or shorter. It is usually possible to look at a graph, decide what is to be kept in and what to be smoothed out (without begging the whole question), and to choose a moving average of length suitable for doing the job. The polynomial method comes into its own in curvilinear regression applied to strictly static data.

Analysis to Eliminate Trend (in Regression Problems)

The phenomenon of nonsense correlation between time series variables is widely appreciated, particularly when it is due to a fortuitous resemblance between simple trends. It may also arise, more subtly, when the series are serially correlated, because any point, being very like its neighbours, tends to tell much the same story, and results in the same sampling errors being counted many times over; this results in standard errors of regression and correlation co-efficients much greater than the standard tests suggest, and consequently in many cases of spurious significance. The method of coping with this is to remove some of the longer term variation and correlate what is left.

The example on the blast furnace achieves this by correlating deviations from six-week averages. If it is reasonable to assume that local variations of the series over periods of this length is effectively random (i.e. the trend does not move much over such a period) the analysis of covariance becomes available as a convenient and valid method of analysis. Deviations from a suitable moving average would give greater sensitivity, but it would be necessary to allow for serial correlation introduced into the data by the trend removal procedure; this takes the method of analysis out of the field covered by text books, but an account of it has been given elsewhere.

Trend removal can be a very good way of getting rid of true correlations as well as spurious ones, and is apt to do this if the variation is very materially reduced by the removal of trend; relations which hold for big movements may not hold proportionately for marginal ones, and the trend removal method is based on the

assumption that the same relationships hold for both large and small variations. If this difficulty does not arise, however, it seems from numerical examination of some typical particular cases that when the moving average method is used, it is best to take the moving average as long as it is safe when the cause variable is smoother than the effect variable in its local variations, and in the opposite event to keep it short. Valid analysis can be performed without the residuals from the moving average being strictly random, but their serial correlations must be allowed for in tests of significance.

Analysis for Relationship (with Several Variables)

In some multiple regression problems, there is an "effect" variable the value of which depends on the effects of a number of "control" variables which have been varied arbitrarily, though not necessarily independently. Sometimes there is an uncomfortably large number of these, and the method described seems as good a way as any of getting rid of the comparatively unimportant ones. It is of course possible to overlook important variables in this way, particularly if one imposes a significance test on the variables before admitting them to the final analysis; but how to avoid this is a difficult problem. If a variable seems unimportant to begin with, but has strong correlations with others which seem important, it might be as well to admit it to final analysis in case its effect has been counterbalanced by that of the others.

In the case of the blast furnace, some of the variables which we wish to include as independent variables in regression analysis cannot be regarded as subject to arbitrary control, and in consequence certain additional difficulties arise, of which two may be briefly discussed.

The first of these arises in the case where two variables overlap considerably in what they measure, as, for instance, 2 in. shatter and 1½ in. shatter in coke; in this case, both are acting as indices of coke strength, and the two together may be expected to reflect this more adequately than either taken separately. If two such variables are both thrown into a multiple regression analysis as independent variables, it may happen that the effect of the underlying property is attributed partly to each of them, and the share may not attain significance in either case, whereas it would if only one were included; thus an important cause may fail to be established as such. Furthermore, if both are included, a difficulty of interpretation arises. If the relationship between the two is statistically strong and stable (as with the two shatter figures in the

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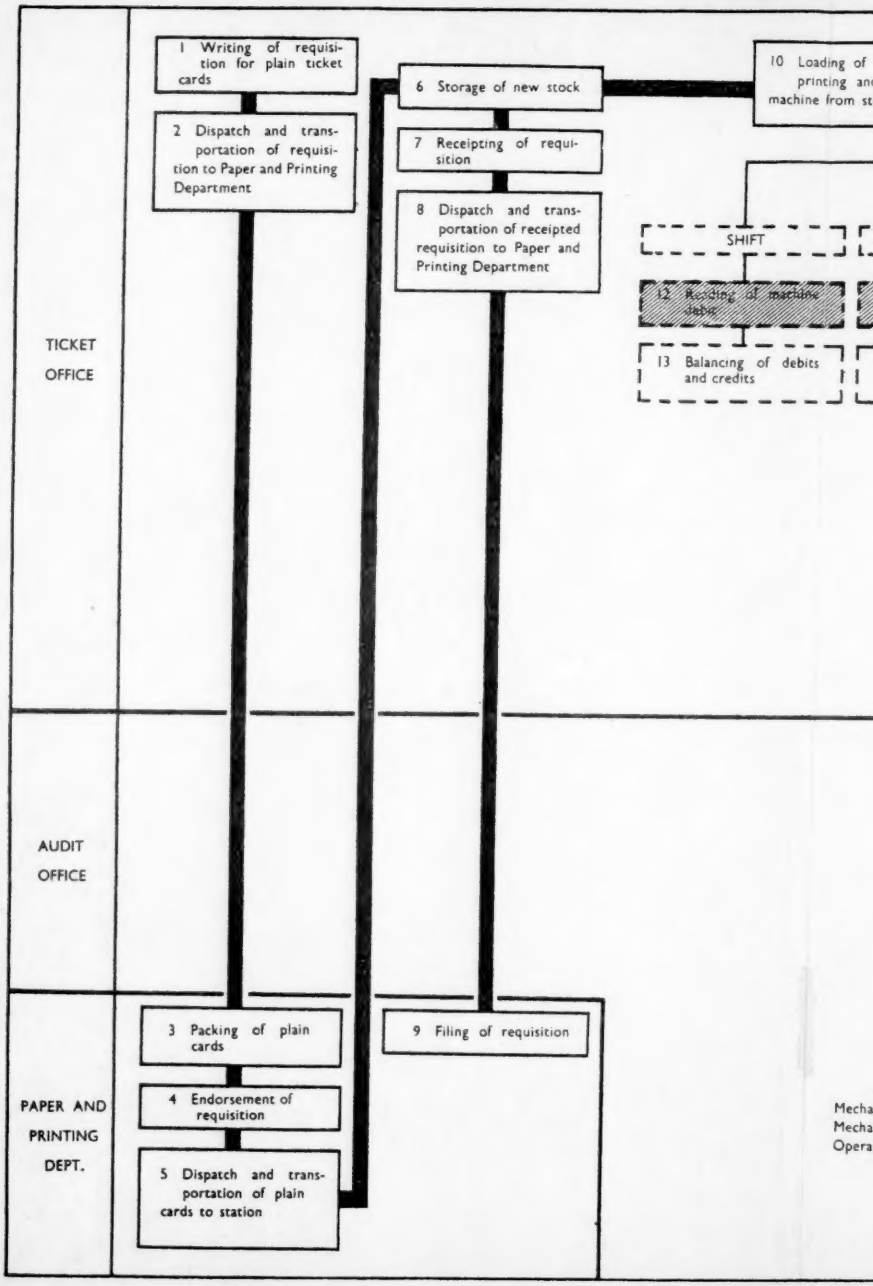
FLOW CHART OF OPERATIONS IN MECHANIZED


TICKET OFFICE
AUDIT OFFICE
PAPER AND PRINTING DEPT.
1 Writing of requisition for plain ticket cards
2 Dispatch and transportation of requisition to Paper and Printing Department
3 Packing of plain cards
4 Endorsement of requisition
5 Dispatch and transportation of plain cards to station
6 Storage of new stock
7 Receipting of requisition
8 Dispatch and transportation of receipted requisition to Paper and Printing Department
9 Filing of requisition
10 Loading of printing an machine from st
SHIFT
12 Reading of machine totals
13 Balancing of debits and credits

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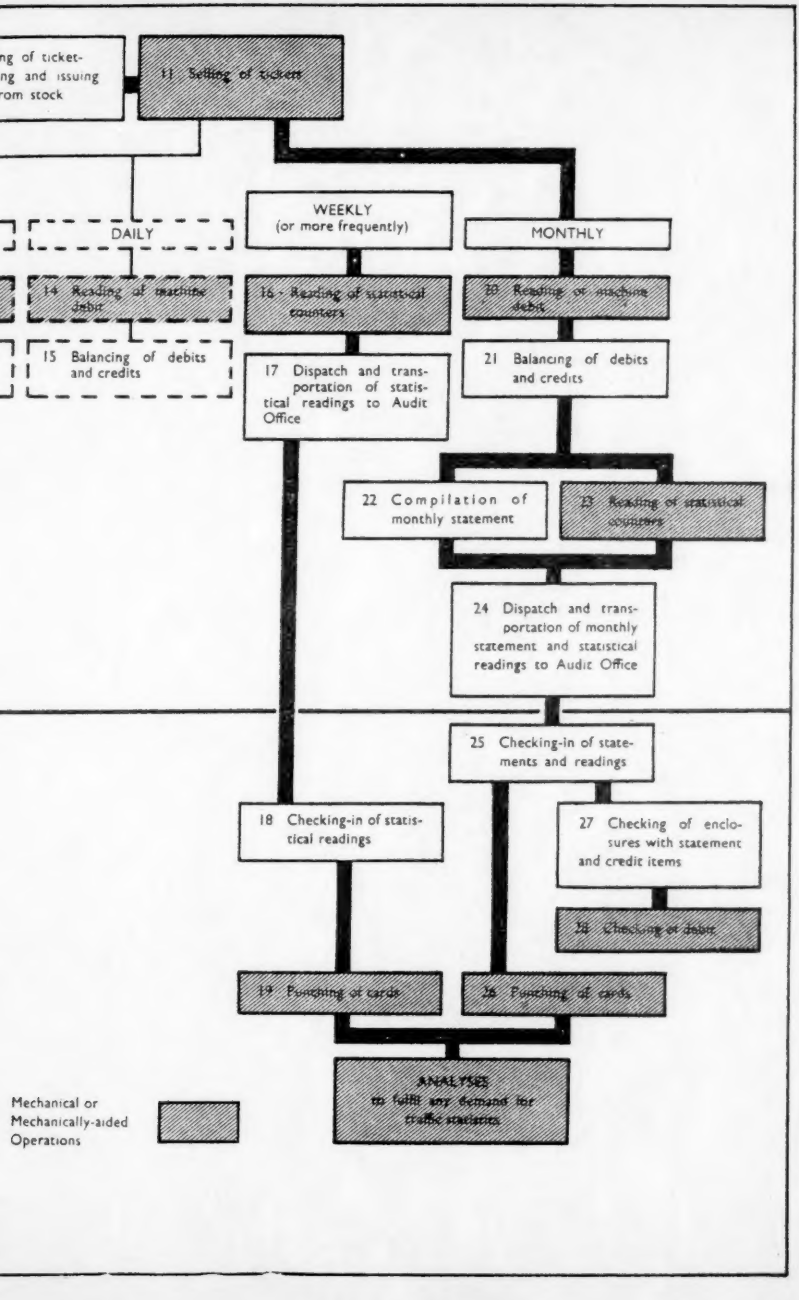
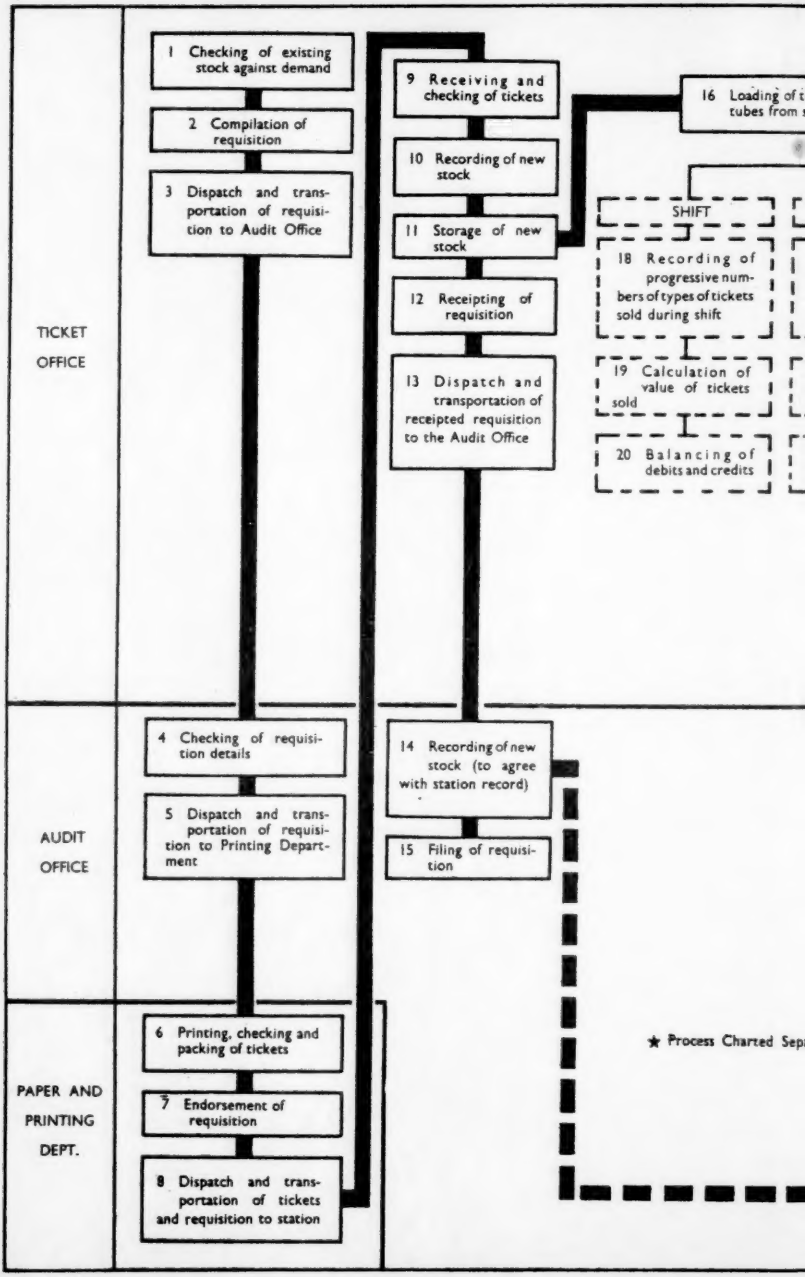
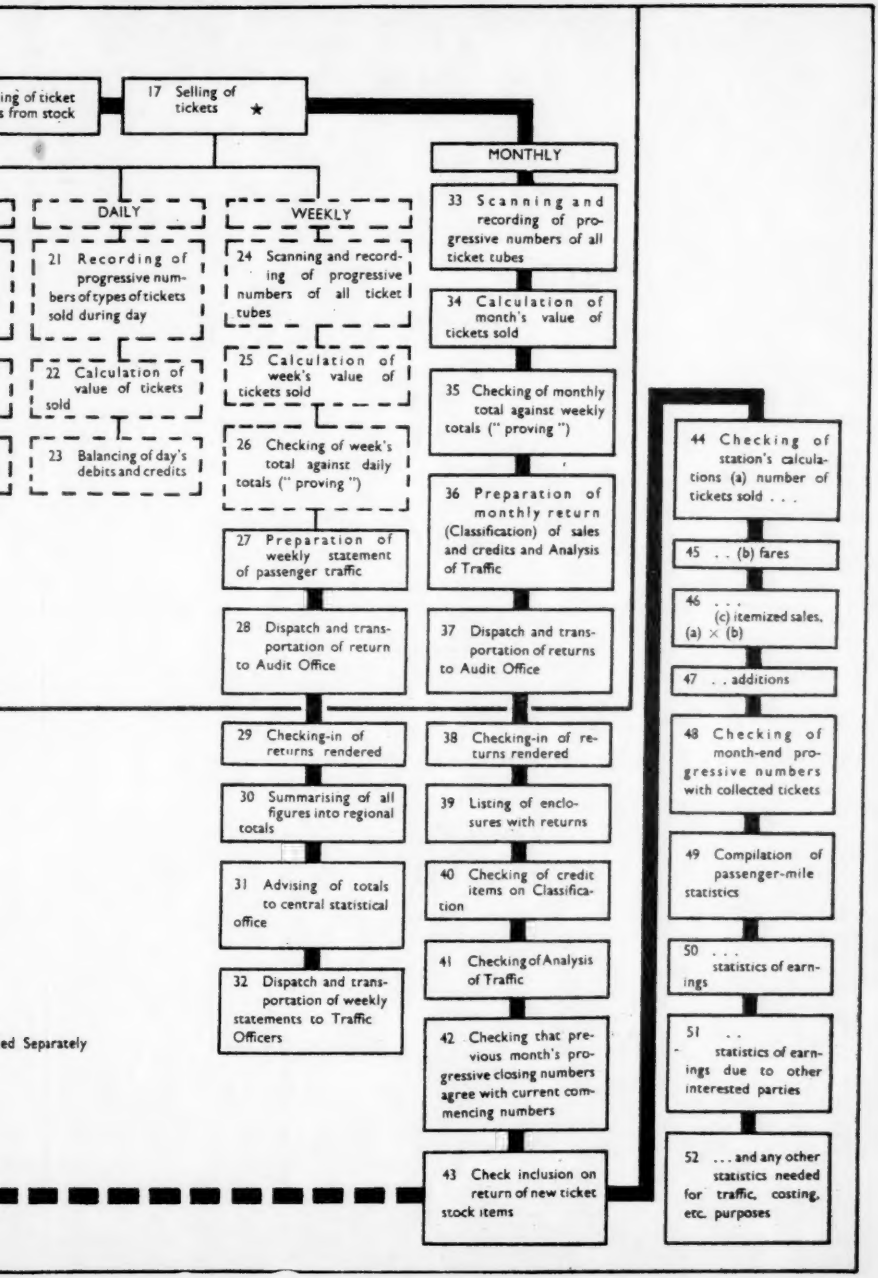
ED TICKET ISSUE AND RELATED PROCESSES


ng of ticket-
ng and issuing
rom stock
11 Selling of tickets
DAILY
WEEKLY
(or more frequently)
MONTHLY
14 Reading of machine debit
16 Reading of statistical counters
20 Reading of machine debit
15 Balancing of debits and credits
17 Dispatch and transportation of statistical readings to Audit Office
21 Balancing of debits and credits
22 Compilation of monthly statement
23 Reading of statistical counters
24 Dispatch and transportation of monthly statement and statistical readings to Audit Office
25 Checking-in of statements and readings
18 Checking-in of statistical readings
27 Checking of enclosures with statement and credit items
28 Checking of debits
19 Punching of cards
26 Punching of cards
ANALYSES
to fulfil any demand for traffic statistics
Mechanical or Mechanically-aided Operations

FLOW CHART OF OPERATIONS IN MANU


TICKET OFFICE
AUDIT OFFICE
PAPER AND PRINTING DEPT.
1 Checking of existing stock against demand
2 Compilation of requisition
3 Dispatch and transportation of requisition to Audit Office
9 Receiving and checking of tickets
10 Recording of new stock
11 Storage of new stock
12 Receipting of requisition
13 Dispatch and transportation of receipted requisition to the Audit Office
16 Loading of t tubes from
SHIFT
18 Recording of progressive numbers of types of tickets sold during shift
19 Calculation of value of tickets sold
20 Balancing of debits and credits
4 Checking of requisition details
5 Dispatch and transportation of requisition to Printing Department
14 Recording of new stock (to agree with station record)
15 Filing of requisition
6 Printing, checking and packing of tickets
7 Endorsement of requisition
8 Dispatch and transportation of tickets and requisition to station
★ Process Charted Sep

ANUAL TICKET ISSUE AND RELATED PROCESSES


ing of ticket s from stock
17 Selling of tickets ★
DAILY
21 Recording of progressive numbers of types of tickets sold during day
22 Calculation of value of tickets sold
23 Balancing of day's debits and credits
WEEKLY
24 Scanning and recording of progressive numbers of all ticket tubes
25 Calculation of week's value of tickets sold
26 Checking of week's total against daily totals (" proving ")
27 Preparation of weekly statement of passenger traffic
28 Dispatch and transportation of return to Audit Office
MONTHLY
33 Scanning and recording of progressive numbers of all ticket tubes
34 Calculation of month's value of tickets sold
35 Checking of monthly total against weekly totals (" proving ")
36 Preparation of monthly return (Classification) of sales and credits and Analysis of Traffic
37 Dispatch and transportation of returns to Audit Office
29 Checking-in of returns rendered
30 Summarising of all figures into regional totals
31 Advising of totals to central statistical office
32 Dispatch and transportation of weekly statements to Traffic Officers
38 Checking-in of returns rendered
39 Listing of enclosures with returns
40 Checking of credit items on Classification
41 Checking of Analysis of Traffic
42 Checking that previous month's progressive closing numbers agree with current commencing numbers
43 Check inclusion on return of new ticket stock items
44 Checking of station's calculations (a) number of tickets sold . . .
45 . . (b) fares
46 . . . (c) itemized sales, (a) × (b)
47 . . additions
48 Checking of month-end progressive numbers with collected tickets
49 Compilation of passenger-mile statistics
50 . . . statistics of earnings
51 . . statistics of earnings due to other interested parties
52 . . . and any other statistics needed for traffic, costing, etc. purposes
ed Separately

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product of a particular coking plant) the idea of varying one and holding the other constant is unrealistic, interpretable only in terms of selection, and so is the corresponding partial regression coefficient. In a case like this, both may be "held constant" in assessing the effect of some other variable, but one should not be held constant in assessing the effect of the other.

A second difficulty arises when some of the independent variables are partial causes of the variation of others, as well as of the variation of the dependent variable. In the blast furnace, for example, coke consumption is supposed to depend on the percentage of silicon in iron (acting as an index of hearth temperature) which is partly dependent on coke burnt per unit time (reflecting rate of drive), itself an important cause variable. To throw two such variables (which we may term secondary and primary control variables respectively) into a multiple regression analysis together may produce partial regression coefficients which are difficult to interpret, and therefore potentially misleading. To vary the secondary variable while holding the primary control variable constant is realistic enough, and the appropriate partial regression coefficient should not mislead; but to keep the secondary variable constant while varying the primary control variable is something which could be conceived of as being done in reality only by the careful balancing of a number of primary control variables in one of possibly many different ways, each with a different consequence for coke consumption, and therefore the corresponding regression coefficient may have no useful practical interpretation.

This sample of two difficulties indicates how the indiscriminate bundling together of variables into a massive regression equation in the hope that the arithmetic will sort things out is not a very safe approach. This also tends to be confirmed by our own and other people's bitter experience. It is safe if in addition to the usual assumptions about linearity of effect, and so on, the independent variables are all control variables, free from causal constraints and correlated only fortuitously, and if the residual is random. Otherwise such regression equations as are used must be built up with careful criticism at every stage, both from the structural and nonsense correlation point of view. It may be necessary to do several regression analyses, giving a causal interpretation to only a few of the partial regression coefficients in each.

The authors wish to thank the Directors of the Steel Company of Wales Limited for permission to publish this paper, and the colleagues who have participated in the work described. Dr. Jowett is Lecturer in Statistics at the University of Sheffield.

ROAD RESEARCH

The following five summaries refer to a selection of the work undertaken by the Road Research Laboratory. *They are reproduced by permission of the Director of Road Research, Road Research Laboratory, Harmondsworth, Middlesex, to whom anyone interested should refer for further information.*

Some Measurements of the Braking Performance of Motorcycles in use on the Roads.

RN/2186/RGS.

Motorcyclists passing along the trunk roads were stopped by the police and the braking performances of their machines were measured. A testing machine which recorded the braking force at each wheel was employed. The main conclusions reached were as follows: (1) 80 per cent of the riders could lock the rear wheel and most of them could do this easily. (2) 30 per cent of the riders could lock the front wheel but most of them had to apply their maximum effort; only 10 per cent could lock the front wheel with a reasonable effort (not more than 30 lb.). (3) The average value of the maximum braking efficiency, expressed in terms of the weight of the machine plus rider, was about 75 per cent for solo motorcycles and about 55 per cent for combinations. (4) There was little difference in the average braking performance of motor-cycles of different size. (5) Motorcycles of post-war manufacture had a slightly better braking performance than those of pre-war manufacture. (6) Experiments with several motorcycles showed that, for a given machine, there was a close relationship between the braking efficiency registered by the brake-testing machine and the average deceleration obtained in tests on the road. There were some differences, however, in the form of this relationship for different motorcycles and further experiments are desirable. (7) A questionnaire presented to the riders of the motorcycles tested showed that many riders said that they did not use their front brakes when making an emergency stop. This, coupled with the fact that considerable skill and practice are required in making an emergency stop on a motorcycle without losing control of the machine, indicates that few motorcyclists are able to make full use of the braking performance of their machines.

Vehicle Speeds on a Straight Section and on a Tortuous Section of Trunk Road A.41.

RN/2257/TMC.IBL.

As part of an investigation into the effect of road layout on vehicle speeds on rural roads in Buckinghamshire, observations

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were made of vehicles travelling along two level sections, six miles apart, of the London–Birmingham trunk road A41. One section is quite straight and the other contains a number of curves; both sections have two-lane carriageways.

It was found that the tortuousness had a large effect upon the high-speed vehicles while low-speed vehicles were unaffected. Private cars, for example, with an average speed of 47·9 m.p.h. on the straight section, were reduced in speed on the tortuous section to an average of 38·2 m.p.h. In the case of heavy commercial vehicles, on the other hand, the effect was very slight, the average speeds being 29·6 and 29·0 m.p.h. respectively. The results for light commercial vehicles and medium commercial vehicles lie between these extremes.

It is shown that for a given speed of travel on the straight section, the speed on the tortuous section was independent of the class of vehicle. The data show that local traffic travelled more slowly than traffic which went through from one section to the other, the average difference in speed being about 2·5 m.p.h.

Accident Rates

RN/2282/RJS.

The note reproduces a paper read before the Second International Course in Traffic Engineering at Lucerne, Switzerland, in September 1954. It brings together and analyses statistical information about road accidents in a number of different countries, including particularly Great Britain and the United States of America, and draws conclusions therefrom.

One of the most noteworthy conclusions is that the total number of road accidents does not increase at the rate that might be expected with the increase in traffic; road users adjust their behaviour to the traffic conditions and thereby introduce a variable that requires separate evaluation. Thus the number of pedestrian casualties per registered motor vehicle decreases as the number of these vehicles increases, but the number of driver and passenger casualties per vehicle varies much less with changes in the number of motor vehicles.

The ratio of the number of vehicle-occupant casualties to pedestrian casualties in any country increases roughly in proportion to the number of motor vehicles.

The proportion of accidents in which only one vehicle is involved decreases in any country as the number of motor vehicles increases; the proportion of multiple-vehicle accidents increases. Similarly, the more highly trafficked a road the greater the proportion

of multiple-vehicle accidents and the smaller the proportion of single-vehicle accidents.

The proportion of accidents at junctions, the proportion of accidents in rural areas and the ratio of casualties to fatalities all tend to increase as the number of motor vehicles in a country increases. The number of damage-only accidents per personal-injury accident also increases with the amount of traffic.

The accident rate, expressed as the number of accidents involving personal injury per unit distance travelled, is lowest on lightly trafficked rural roads and highest on shopping streets; it is much lower on motor roads than on ordinary roads. The *fatal* accident rate varies, however, quite differently, being higher on rural than on highly congested urban roads, but higher on heavily trafficked roads with much roadside development than on motor roads with restricted access, flyover junctions and dual carriageways.

Parked Vehicles in "no-waiting" Streets in the Inner London Area, February–March, 1954.

RN/2262/HG.

Counts of parked vehicles were made during February and March, 1954, in a number of "no-waiting" streets in the Inner London Area. When the restrictions were first applied in May 1947, the number of parked vehicles was reduced by about 60 per cent but in February–March, 1954, the number was only 20 per cent lower than before the ban. Commercial vehicles alone outnumbered the total vehicles parked just after the parking ban came into operation in 1947.

The lowest number of parked vehicles per mile of road was found in the main traffic arteries such as Oxford Street, and the highest in the side and back streets, especially in the Piccadilly area.

Commercial vehicles are allowed to load and unload in such streets for 20 minutes, but it was found that 53 per cent of the vehicles parked were private cars. Compliance with the regulations might therefore have reduced the amount of parking by at least a half.

A Study of the Time Lost in Parking a Car in the West End of London (1954).

RN/2881/JI.

Observations were made of the times taken by a driver visiting various addresses in the West End of London to find parking place, to park his car, and to walk to and from his destination. Observations were made during week-day business hours of parking both on the street and in garages.

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In the case of street parking the greatest part of the loss of time averaging over four minutes, occurred in walking between the street-parked vehicle and the chosen address of call, a distance of about 1,400 feet. Three minutes were lost in cruising about the streets in the vehicle, in search of the first available parking berth in an unrestricted street. The times required for actually parking and clearing a car from the kerb were both under half a minute.

Parking and clearing times in the garages, and walking times between garage and destination, were considerably longer. Excluding the time taken to find a parking place, the time spent when using a garage was nearly twice as long as when parking in the street.

The results point to the importance of making garage facilities easy to reach and to use.

MECHANIZING TICKET PRINTING AND ISSUE*

by

R. J. EATON

THREE SPECIAL FEATURES distinguish the retailing of the intangible service of railway passenger transport from the retailing of a commodity or product. The first is the urgency imposed either by the transport undertaking or the passenger. The second is the very large number of "lines" or items available for sale—at King's Cross main ticket office for example, 2,500 types of pre-printed tickets are kept ready for sale. The third is the need for relatively frequent balances of stocks and cash for accounting and statistical reasons. Such balances take place at shift, day, week and month ends. Owing to the necessity of checking quantities of progressive numbers carefully and calculating values this is a tedious business. An individual clerk's balance may occupy up to one eighth of a man-day. At one station serving a certain county town 2,900 balances are struck in a year, expending over 2,000 man-hours.

Existing Method

Ticket offices are usually rectangular. On the inside of the front wall there is a counter, on which (except where there are ticket

* Slightly abridged from paper given to Operational Research Society, 15 February, 1955.

windows) stand ticket racks. There may be other ticket racks behind, or at the side of the clerk, necessitating journeys of from 2 to 12 paces of motion. Each rack contains a number of "tubes" from which the operator extracts pre-printed tickets which he dates in a manual "Edmondson" press before handing them to the customers. In addition to tickets contained in the racks against current demand, there is a main stock in continuity with the current stock, stored in drawers and cupboards and on shelves. The stock of each type of ticket is independently, progressively numbered.

There are on British Railways 5,600 selling points (excluding agencies). A recent sample investigation indicated a total stock of 527,000,000 tickets maintained continuously.

The first stage of research into the problems of ticket issue was the construction of a detailed method flow chart (figure 1) showing the operations, not only in the ticket office, but also in the various relative or contributory departments concerned with the stocking and accountancy control of the ticket office and the provision of statistics.

With this chart it was possible to examine the number and routing of the steps involved. No attempt was made at this stage to analyse processes or go into any step in detail. There was the broad outline and it was examined in a broad fashion in order to discover what was strictly productive and what was non-productive.

In an inter-related succession of separate operational paths (which the chart records) there could be difficulty in identifying non-productive work. For, if in any one division of the work some result is achieved, it cannot be said that the labour contributing to it is non-productive within the work-division, especially if the contributory labour is properly organized and used to capacity. In all such cases the researcher has to bear firmly in mind the prime, essential functions which the whole of the method seeks to achieve.

As to ticket issue and control, these essential functions are simply three:

(1) the selling of tickets to customers upon demand and collection of the charges for them;
(2) the recording and auditing of sales and receipts;
(3) the production of traffic statistics at any time in the quickest possible time.

On examining the chart the first facts that strike the observer are:

(1) in the ticket office operations there are three disembodied blocks of sequences (headed "shift", "daily", "weekly") which do not play a part in the main sequence of operations;

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(2) the preponderance within the main sequence of operations in the audit office (23) compared with those in the ticket office (15) and the printing department (3);

(3) the very small opportunity, apart from the actual pre-printing of tickets, of using mechanical aid (except adders and calculators in, for example, operations 46 and 47) in the whole chart.

One recognizes in fact 1 that the non-main sequential operations are, in fact, the "balancing" occasions already described, and the "proving" that earlier balancing has been carried out. The sole purpose of all these operations is to ascertain how much is the sales debit against which must be set cash and credits. The weekly operation does, however, contribute to the compilation of a weekly traffic return.

In fact 2 it is clear that many of the audit operations are merely checks upon station calculations—a still further proving that previous balances and provings have been properly done.

In these modern times it is reasonable that research into productivity should turn naturally to the possibilities of mechanization, (a) to increase the productivity of the individual operator and reduce or eliminate human fatigue; (b) to do automatically what would otherwise require a number of manual operations to achieve; (c) to overcome failure or slackness in the human element; (d) to provide a means of ensuring maximum employment of capital expenditure.

Types of Office

Examination of passenger traffic, its characteristics, rate of handling and so on by means of a sample check of nearly 4 per cent of the stations on British Railways, showed that, in spite of the fact that no two stations are alike from the operator's point of view, there are certain common characteristics which make a coarse classification possible. Further observations at a smaller number of different stations confirmed this, and what is called the "scope system" of classification was evolved. Three "scopes" of ticket office are recognized thus:

Scope 1. Intensive bookings of a few types of tickets to a small number of destinations.

Scope 2. Frequent, but not intensive, bookings of many types of tickets to widespread destinations.

Scope 3. Infrequent or occasional bookings of certain types of tickets.

The work of a particular ticket office might fall within one, or two, or all three scopes.

Mechanizing the Scopes

Scope 1. London Transport pioneered the development of a machine for the rapid issue of up to ten types of tickets upon demand by the pressure of an operating button. A ticket is printed on plain card, cut to size, dated and delivered almost instantaneously to the passenger by means of an endless delivery belt.

British Railways research resulted on the one hand in an increase of capacity of this type of machine to 25 ticket types and, on the other hand, to simplification of the design and mechanism to provide capacities of from one to six types within standard case-works. The installation of the smaller, cheaper machine can often be justified at a particular station where a larger machine could not.

The development of the rapid printer as the outcome of collaboration between British Railways, London Transport and the makers can be regarded as complete. A good many of the machines have been installed to serve scope 1 requirements, particularly around London, Liverpool and in Glasgow and Newcastle.

Scope 2. As already expressed, a mechanized scope 2 requires the printing and issue upon demand of any one of some hundreds of types of tickets and the provision of mechanical accountancy records at the time of issue.

Several continental manufacturers have produced multi-issue machines which claim to fulfil all requirements of scope 2. But British practice is, in some ways, more exacting than the continental and some of the machines are patently unsuitable for British use. Others, however, are almost perfectly suited for adoption. One suitable machine stores within itself up to 1,200 printing plates. It prints, dates, progressively numbers and ejects in less than a second any one type of ticket selected by the operator, prints details of the issue on an itemized record, and adds the value of the ticket to a totalizer. This totalizer is at all times available to indicate the cash total of sales throughout a given period. The machine also provides a statistical record of issue in respect of each of the many types of tickets it is capable of issuing.

Scope 3. A third type of machine, now being perfected, is suitable for infrequent bookings. While it is similar in operation to the scope 2 machine previously mentioned, it employs extraneous matrices, one of which has to be inserted to print a ticket. But it has the great advantage of being able to print both passenger tickets of conventional size and season tickets, which are larger.

This machine also provides mechanical sales and issue records but each matrix has to be "read" for statistical purposes because it would be uneconomic to include special internal mechanism.

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It is possible, therefore, to mechanize all three scopes of ticket issue by means of either rapid-issue or multi-issue machines and, as the capabilities of these are known, to construct a revised flow chart of the operations in a mechanized system (figure 2).

The striking features of the revised chart are: (1) the reduction in the number of operations by almost half from 52 in the manual system to 27 in a mechanized system; (2) the relieving of the audit office of much checking work and the reduction of its operations from 23 to 7; (3) the number of operations in which the machine plays a part.

The saving of time, represented by the eliminated operations of the whole manual system, and the operating time of all the substituted mechanical operations, are not yet known because, as already stated, research is not complete. But certain savings can be forecast.

In the ticket office, for instance, the "balancing" operation could be reduced from the 30 to 45 minutes now taken to possibly five minutes. The ticket-issuing machines would show the amount of sales at any moment and take but a minute or two to print.

At week-ends and month-ends all manual book ledger calculation (occupying many man-hours) would be substituted by machine readings and the printed records sent to a central office where a punched-card system could be used for computation and analysis.

In the audit office there would be, primarily, a saving in the work of controlling the ordering and supply of pre-printed tickets and a considerable economy as the outcome of station itemized returns being abolished. A large audit office staff has to be employed on checking under the present system. Such operations as the confirmation that a closing number last month has been correctly brought forward on a station's returns as a commencing figure during the current month ("continuity of record") can be done only by the human unit—for a scanning, detecting machine would be so costly as to be out of the question.

At the present time a truly terrific amount of man-hours, much of it at a higher overtime rate, is expended on the extraction of post-ordered statistics. (Regular statistics are often not of the right type for mere summation in special cases.) The rapid machine reading of dissection, and the machine's printed record, would enable punched-card analyses on every conceivable basis to be furnished promptly and currently at a time when the statistics are "fresh" and of maximum assistance to operations.

All these matters, and other cognate items, will be measurable during the full research experiment yet to be made. We have been encouraged by the management to get out plans for a controlled

test to see how much economy would be achieved in practice by full mechanization and arrangements are in hand.

Process Studies

In the meantime comparative studies have been made of certain ticket-selling operations and process charts compiled.

Scope 1. In booking customers within scope 1 it is usual, in order to save time, to pre-date a quantity of one or two types of tickets. These lie upon the operating counter in a heap or heaps, from which the operator picks a ticket as necessary. The number pre-dated in relation to the number sold can never be accurately estimated; sometimes too many tickets are dated and the waste must be cancelled, sometimes too few are dated and the operator must then date each further ticket as issued. Obviously, not all the types of tickets saleable within scope 1 can be pre-dated for this would require an unmanageable number of heaps of tickets.

In five consecutive typical transactions within scope 1 of the manual system, the time of operation varied between 3·3 sec. and 8·0 sec., with an average of 6·0 sec. It was noticeable that the operation of extracting a ticket from its tube took at least 2 sec. plus ·8 sec. in dating it plus 1 sec. in delivering it (a total of 3·8 sec.) compared with 1·5 sec. for the combined operation of picking up a pre-dated ticket and delivering it to the customer. This fact is a pointer to the situation in the whole transaction where, mechanically, a ticket requires to be printed, dated and ejected in the fastest time.

So fast, in fact, are rapid-issue machines in operation that it is impossible to time their use by the usual "therblig" symbols and, owing to the circumstances of operation (which include public participation) micromotion photography is undesirable and impracticable. Instead, a table is constructed to show minute-by-minute performance, with a record of retarding factors. Such a table is reproduced as table I. It relates to one of four machines at a suburban "dormitory" terminus whence the passengers travel to the City and intermediate stations to their places of employment. The specimen table covers a period during which special cheap "early morning return" tickets are available.

The push-button machines respond instantly to the operator's action but obviously (except for pre-printing tickets, if necessary, to sell elsewhere) a machine can never be operated at its maximum output of 240 tickets per minute owing to (a) the physical limitations of each passenger who must approach the ticket window, speak his order, lay his money, pick up his ticket (and, in some cases, also pick up change) and then depart towards the train; (b) pauses of various

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TABLE I

MINUTE-BY-MINUTE PERFORMANCE OF SCOPE 1 MACHINE

Station: Suburban "Dormitory" Terminus, 10 miles from City centre.
Period of Observation: Friday, 7.0 to 7.30 a.m.
Machine: One of four Rapid Printing and Issuing.

Minute No.	No. of Passengers Booked	Accumulative Number of Passengers	Average No. of Passengers per minute (10 minute periods)	Average No. of Seconds per Passenger (10 minute periods)
1	13			
2	10	23		
3	10	33		
4	8 (a)	41		
5	9	50		
6	10	60		
7	9	69		
8	10	79		
9	8	87		
10	12	99	9·9	6·06
11	14	113		
12	12	125		
13	11	136		
14	12	148		
15	14	162		
16	7 (b)	169		
17	11	180		
18	11	191		
19	16	207		
20	18	225	12·6	4·76
21	17	242		
22	18	260		
23	13 (c)	273		
24	20	293		
25	16	309		
26	18	327		
27	16	343		
28	15	358		
29	25	383		
30	14	397	17·2	3·48

Average number of passengers per minute overall: 13·23
Average number of seconds per passenger: 4·53

(a) Passenger tendered paper money.
(b) Passenger dropped and retrieved coin from floor.
(c) Two change-giving delays.

lengths between passenger approaches. These delaying features are demonstrated in the table. One can see that just after seven o'clock the number of passengers is comparatively sparse; that from the 19th minute there is an acceleration until the 27th minute. This is

due to the fact that the special cheap tickets are not issued after 7.30 a.m. and therefore the passengers have an acceleration incentive. During the crucial 29th minute as many as 25 passengers were booked.

During the period of study a further breakdown of two random minutes was made: one at the 19th minute, the other at the 24th. The 19th minute showed a steady, uneventful flow except during the seven-seconds span from seconds 25 to 32 when a slow-moving passenger delayed those following. There was an indication, during the next span (33–34) of the next following passenger's impatience or his readiness to pass the ticket window quickly. During the 24th minute there were two cases in which the customer was booked in one second. This is explained by the fact that occasionally an impatient passenger will, while the previous customer is picking up his ticket, shout his order and lay the exact fare into the ticket window before actually reaching it and before the previous passenger has moved on. There has thus been an overlap of two transactions.

Another random minute recorded at a station in central London, serving an area with a heavy "business" population at 12 noon on a Saturday, showed a marked rhythm and rate of flow, indicating the quantity of home-going passengers laying the exact fare, compared with the irregular rate in the suburban 24th minute in which there are change-giving retardations on several occasions.

The random minute timings are interesting as demonstrating what can be achieved within scope 1 by mechanical printing and issue. So far as the operator is concerned there is no physical reason why, for instance, a 3-sec. or less headway between passengers should not be maintained. An important advantage of rapid-printer operation is that the operator does not move from a set position at the operating panel of the machine. If this panel is fixed at a position close to the left hand it enables fully balanced utilization of both hands to be achieved.

Scope 2. A typical series of operations in a scope 2 office varied in time between 6·8 secs. (exact fare tendered, no operator travel), and 13 secs. (change required and a 10 ft. travel). The average of 4 times in this particular series of observations was 10·82 secs., and the average travel was 7 ft. Observations were made of a demonstration machine recently imported from the continent. But as the operator was unpractised, the results cannot be regarded as authentic traffic performance. There was, in fact, little difference in average distances and times. The mechanical system required 5 ft. and 11·02 sec. The 5-ft. distance is, however, constant, representing the movements of the operator (which must be made for every booking) from the ticket window to the operating position at the machine and

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back to the window. But, as explained, the operator was not fully skilled. Completely reliable timings can be made only during tests it is hoped to make under actual traffic conditions.

Customer Delay

Finally, it should be mentioned that any conclusions drawn from this work must be subject to the fact that there is a substantially uncontrollable customer delay factor. This ranges from the minimum in respect of an agile (or impatient) person, giving a distinct order and presenting the exact fare money, to the maximum in the "helpless" type of passenger who mumbles and fumbles and dawdles at the ticket window. Further research may lead to a reduction of this retarding effect.

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OPERATIONAL RESEARCH AND THE RAILWAYS

An article in *Research* ("Research and the Railways", *Research*, March, 1955, **8** (3), 106–112) discusses the Railway Development Plan and considers the research facilities available within the British Railways organization. It particularly emphasizes the key position of operational research. "The Operational Research Division is by comparison very small. It consists of its superintendent and only four men and three technical assistants. But its size is misleading, for, in many respects, the operational research division is the most important."

The conclusion is drawn that "there seems to be very strong grounds for an immediate expansion of the Operational Research Division with men of suitable outlook and experience".

ISOPLUS n

Interpolating an Additive Cycle in a Multiplicative Multistage Nomograph

by

STAFFORD BEER

MODERN PRACTICE IN COMPUTATION techniques has a strong bias toward complete generality. Most of our digital machinery, from the fully automatic desk model to the costly electronic computer, is capable of comprehensive calculations; it is designed to tackle any problem. Analogue computers also exist, from the slide-rule to the differential analyser, but these also mostly aim at a modified generalization. There are many situations, however, in which a single but fairly complicated expression has to be worked out many times a day for an infinite number of combinations among its variables. It is by no means economical to use a digital method over and over again to produce these solutions: the best course is to "freeze" the pattern of the expression or formula in a predetermined system, through which figures can then be moved without further thought.

This object can be achieved by a multistage nomograph: an *ad hoc* geometric arrangement of scales which works as an analogue of the basic laws of arithmetic and of the relationship of the variables in the problem formula. Figure 1 shows a nomographic arrangement to solve expressions of the type kp/q. The advantages of this method of calculation are many. Anyone who has the values of the variables can solve the equation without any thought; there is no need to calibrate the intermediate axes (such as kp in figure 1); the method is quicker than any digital method available.

Some objectors have felt that serious inaccuracies will arise, because a graphical error made at an early stage will be operated on throughout the calculation and might become enormous. To overcome this objection, I recommend mounting the nomograph under glass, and charting a calculation across it with two Perspex rules. If these are made in the shape of a trapezium, with very sharp points, the plot can be made very accurately indeed. (By this means, I have consistently obtained accuracy to within 1 per cent on a nomograph with as many as eleven cycles.)

There is a much more serious objection, however. For an expression dealing in the addition and subtraction of its variables, an ordinary scale in arithmetic progression of equal intervals must be used on the nomographic axes. But for an expression dealing in

products and quotients, these scales must have equal intervals in geometric progression: that is, the scales must all be logarithmic. Hence, if the expression requiring solution intermingles the two sorts of arithmetic, a single nomograph cannot carry the calculation straight through.

This was precisely the practical difficulty that led to the present work. A method had been devised by which to forecast the length of time which would be taken to process any material on any machine in the course of the manufacture of cold rolled steel strip. This forecast could be made on the basis of an operational research model of processes, productivities and statistical trends, and could be operated at three different levels of validity. Separate statistical criteria were provided to give this forecast at these different levels, which were: immediate (expected), long-term (optimum), and ultimate (maximum).

The calculation problem involved the solution of an equation in nine major variables, the penultimate step being the *addition* of a handling factor. Following a final multiplication, the result had to be weighted for the forecast required, using the reciprocal of one of eighty statistically controlled indices selected at the desired level of validity. Thus two multiplicative steps followed the addition.

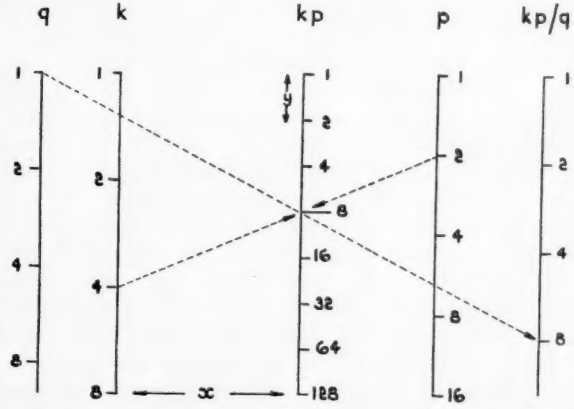

FIGURE 1. *Two stage nomograph to find* kp/q. (*Example worked for* k=4, p=2, q=1.)

Key dimensions:

Let the vertical graphic distance of the geometric interval on k *axis* = a *units, and on* p *axis* = b *units. Let the horizontal distance between the scales* k *and* p = c *units.*

Then $x = c(a/a+b)$

and $y = ab/a+b$

The machine-job forecasts were for use in production control. It was expected that the equation representing this O.R. model would have to be solved by a staff of planning clerks, unskilled in special mathematical procedures, perhaps a hundred times each day. For the reasons given above, a nomograph seemed to be a much better, as well as a much cheaper, tool than a calculating machine for the job. And it was at this point, four years ago, that the following technique was devised. The prototype nomograph embodying this additive cycle has been in use ever since.*

The technique as now described is taken from its historical context, and presented in a completely general form. The difficulty is formally stated; the solution is given in practical terms, tables for its application being supplied in an appendix. Geometric proof of the projections is then given and the equation on which the table is constructed is elicited.

The Problem

The expression $k(abcd/efgh)$ is complicated, but can readily be set up as a multistage nomograph with logarithmic scales. On the final resultant scale appears the numerical value of this expression, called kp. Let it now be necessary to add n to kp, and to divide the result by q. This is impossible by orthodox means, without reading kp off a first nomograph, digitally adding n, and dividing by q on a second nomograph. Our problem is to find a means of calculating on a *single* nomograph an expression of the type $(kp+n)/q$.

Since an addition has to be made, there can be no doubt that the original scale's geometric intervals will have to be transformed to arithmetic intervals: this will be the basis of any solution. The simplest method by which this may be done is to draw an arithmetic scale at right-angles to the original geometric axis, and to plot the logarithmic curve given by these two co-ordinates. An arithmetic projection may then be made by referring a point on the geometric scale horizontally to the curve, and dropping a perpendicular from this point to the arithmetic scale. The reason why this result cannot be adapted to meet the conditions for an additive cycle recounted in the introduction, serves to elaborate the problem as it appeared.

One of the main advantages of nomography (when unskilled clerks are handling the computations) is that effort and error are both minimized by *ad hoc* calibration of the scales. For example, we had to deal with some eighty machines, each having its own absolute operating speed. Instead of treating speed as a continuous variable

* The author has given a detailed description of these briefly recounted circumstances in *Applied Statistics*, **4** (1), 1955.

on a fully-marked scale, it was much preferable simply to mark the relevant axis in eighty places, labelling those positions with machine names. A similar argument applies with the handling factors which had to be added. There was a discrete number of these factors; and it was most desirable that these should be designated separately on the nomograph, and that each should be labelled with its appropriate list of machines.

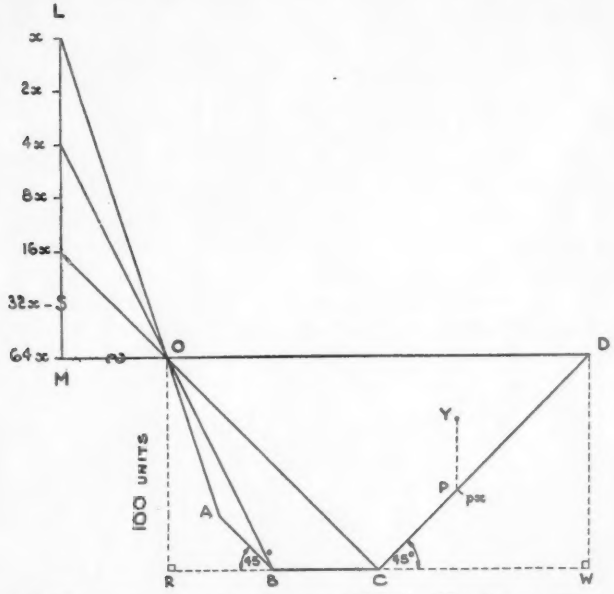

FIGURE 2. *The construction of isoplus zero* (ABCD). (*The distance* PY *for given* n *is obtained for successive values of* p *from the table.*)

Now the adaption to additive purposes of a logarithmic curve projection of the geometric axis (described above) would thwart these intentions. The projection would hit the arithmetic scale at an arbitrary point. Thus the operator would have to use a separate list of additive factors: they could not be marked nomographically. Moreover, the projected scale could not even be marked as a continuous variable from such an arbitrary point: the operator would have to consult his list, and then count the calibrations. This practice would contravene all the principles we had evolved for these nomographs. What is s..aightforward to a scientist using a nomograph to evaluate a mathematical expression, could be a source of embarrassment and continual error to a planning clerk.

Thus, in proceeding to the solution of the additive cycle $(kp+n)/q$, this special constraint should be borne in mind. It complicates the basic nomography, but simplifies the practical application.

The Solution

When the earlier multiplicative stages of the nomograph have been constructed, a resultant line representing kp will be reached. To any point on this line we wish to add n.

Let the scale LM in figure 2 be this resultant kp, logarithmically scaled. The factor x is incorporated to give the scale complete generality. Construct the remainder of figure 2, in which $MO = TM$, $OR = 100$ arbitrary units of distance chosen for convenience, $A\hat{B}R = D\hat{C}W = 45°$. Construction lines may now be removed, leaving merely the scale LM, the dot marking the position of O, and the jointed line $ABCD$. On this line, the value x is attributable to point A, $4x$ to point B, $16x$ to point C, and $64x$ to point D. AB, BC, CD should each be scaled arithmetically and independently by dividing them into equal intervals, to any degree of detail desired.

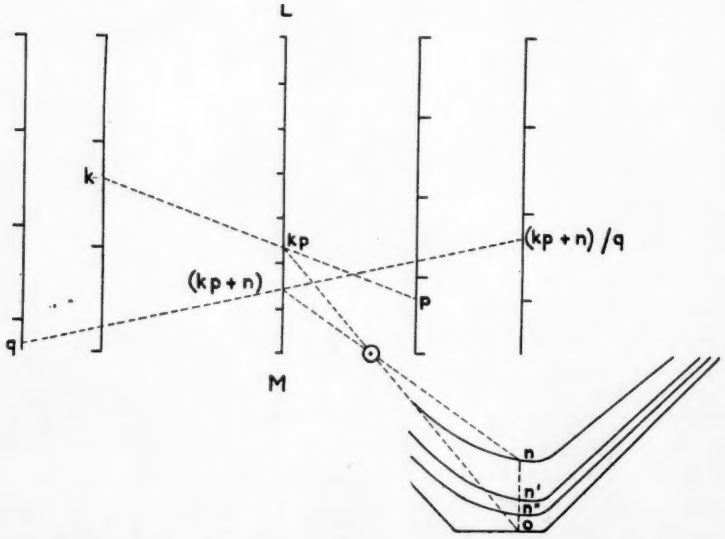

FIGURE 3. *Nomographic route for the solution* (kp+n)/q. (*Showing isoplus zero, and the isopluses* n, n′, n″.)

Let any point on $ABCD$ be P, having a numerical value px, and let the number which has to be added be n. Consulting the table of "Isoplus n" we may find the length of PY for that p and that n, expressed in the same arbitrary units of distance as were used in constructing OR. PY should now be plotted for sufficient values of px to obtain the locus of Y. This line joins all the points of equal n addition, and I have therefore called it an "isoplus". The process is repeated for each value of n which it will be necessary to add into the total calculation, and the resulting drawing, from which all construction and calibration may now be removed, will look like figure 3 (in which the logarithmic cycles of figure 1 have been included).

Once this section of the nomograph has been drawn, the method of operation is simple. A calculation has been plotted across the multiplicative nomograph as far as the axis LM, where the value kp is reached. A ruler is then placed from this point, through O, to the $ABCD$ base line (isoplus zero). From that point of intersection, a vertical line is carried up to intersect an isoplus for the required value of n; and from this intersection, back through O onto LM. The new point reached on LM now has the required value of $kp+n$. This simple "triangulation" is plotted on figure 3.

From this additive operation, which is recorded on the original logarithmic scale LM, it is easy to continue with further nomographic cycles to find products or quotients as required.

The Proof of Isoplus Zero

(i) In figure 4, $ABCD$ is isoplus zero ($A\hat{B}C = B\hat{C}D = 135°$).

(ii) The scalar properties of AB, BC, CD as projected through O from LM are equivalent (by similar triangles) to the scalar properties of ZX, XE, EM. This holds independently of the length of OD, which is arbitrary.

(iii) It can be shown by similar triangles, and particularly those shaded in figure 4, that:
$MN = 2NE;\ EK = 2KX;\ XU = 2UZ.$

(iv) Imagine that points representing successive integers on TM are joined to O. It is seen from result (iii) and its corollaries that whereas the scalar interval between these points diminishes from T towards M, the projected scalar intervals on EM are all equal. Similarly, both XE and ZX are internally divided into equal intervals.

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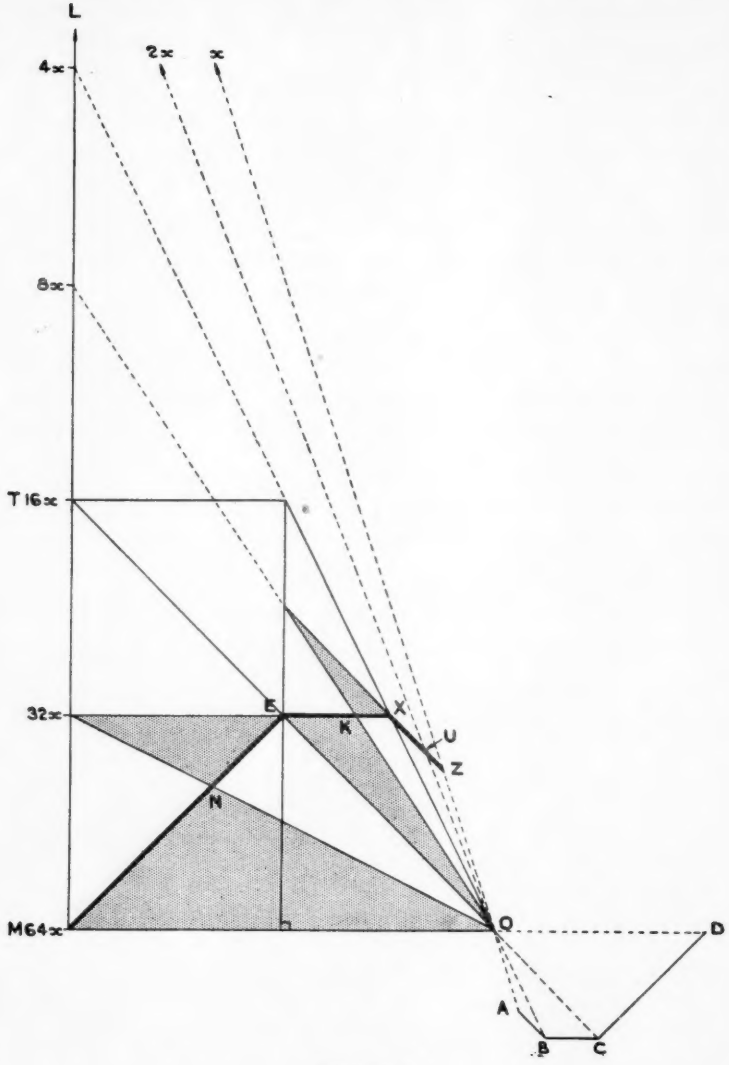

FIGURE 4.

(v) Thus from (iv) and (ii) we find that when the continuous scale on *LM* is divided in logarithmic intervals, the

projected scales on AB, BC and CD are divided (independently of each other) in arithmetic intervals.

(vi) *Note:* (See figure 2). This demonstration of the nature of isoplus zero is subject to the constraint $TM = MO$, which determines the particular angles $A\hat{B}R = D\hat{C}W = 45°$. If this constraint should be graphically inconvenient, it can be removed. A similar geometry to (4) can then be developed for $TM \neq MO$, in which case the trigonometrically calculated angle for $A\hat{B}R = \theta$ is given as:

$$\theta = \tan^{-1}(\alpha/\phi\alpha)$$

where length $TM = \alpha$

length $MO = \phi\alpha$

The Equation for Isoplus n

In figure 5, for the constraint $TM = MO$, by Proof (4):

(i) AB is divided into $3x$, BC is divided into $12x$, CD is divided into $48x$ } equal divisions, projecting from LM through O all G.P. values on scale.

(ii) p and q are any values on LM such that:

$$q - p = n \quad (p < q)$$

(iii) Lines from points representing p, q on LM are projected through O to meet isoplus zero at P, Q. The perpendicular JP from OD cuts OQ at Y.

(iv) Then the locus of Y is isoplus n. (See also figure 2.) This locus is determined for any n by the variable distance PY.

(v) *To find a vertical distance on LM*

Let the graphical length between:

$10x$ and x $= k$

$2zx$ and zx $= l$

p and q $= m$

Since

$$\log 2zx - \log zx = \cdot 3010$$

$$l = \cdot 3010k \qquad (1)$$

$$m = k\ (\log q - \log p) \qquad (2)$$

(vi) *To find PY*

$$MT = 2(\cdot 3010k) \qquad (1)$$

$$\therefore MO = \cdot 6020k \qquad (MT = MO) \qquad (3)$$

$$m = GF \qquad (OM = OH \text{ constructed})$$

$$\therefore GF = k(\log q - \log p) \ (2) \qquad (4)$$

Let $OR = c$

Then

$$c = DW = CW = RC = 2BC = 4RS = 4SB \qquad (5)$$

(From figure 4)

Consider *CD:*

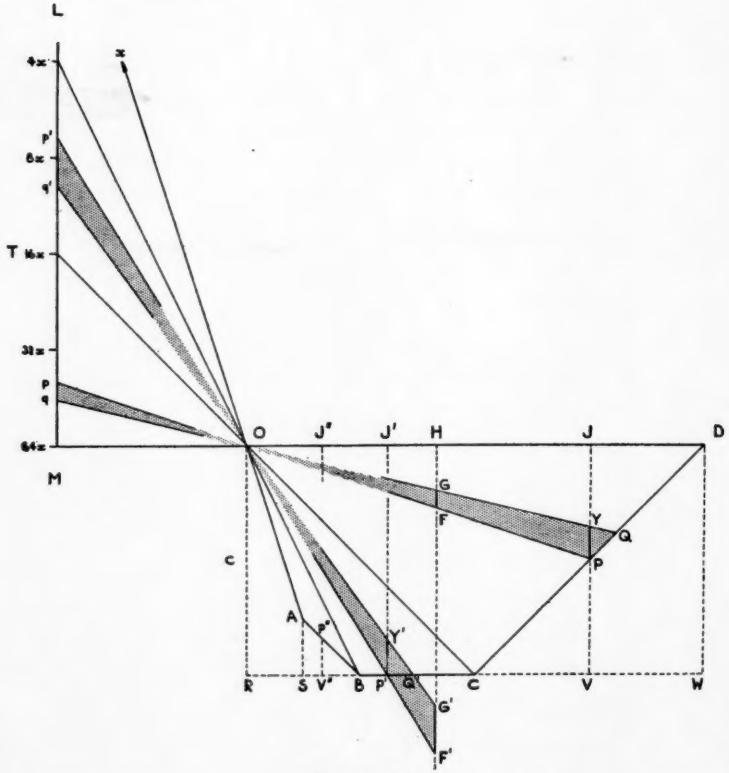

FIGURE 5.

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Using data 5 (i), CP has the scalar value $(p-16x)$

$CW = c$ (5)

let $d = c/48x$ (definition on CW)

Then

$$CV = (p-16x)d \tag{6}$$

Similarly on BC:

$BC = c/2$ (5)

$$\therefore BP' = (p'-4x)/2d \tag{7}$$

Similarly on AB:

$SB = c/4$

$$\therefore SV'' = (p''-x)/4d \tag{8}$$

Consider OJ:

$$\begin{aligned} OJ'' &= RV'' = RS + SV'' = c/4+(p''-x)/4d \text{ (5, 8)} \\ OJ' &= RP' = RB + BP' = c/2+(p' - 4x)/2d \text{ (5, 7)} \\ OJ &= RV = RC + CV = c +(p -16x)/d \text{ (5, 6)} \end{aligned} \tag{9}$$

Consider Ratios:

$GF/PY = OF/OP$ ($\triangle OFG$ similar to $\triangle OPY$)

$OF/OP = OH/OJ$ ($\triangle OFH$ similar to $\triangle OPJ$)

$OH/OJ = OM/OJ$ ($OH = OM$ constructed)

$\therefore PY.OM = GF.OJ$

$\therefore PY = GF.OJ/OM$

For GF see (4), OJ see (9), OM see (3)

Then

$PY = k(\log q''-\log p'')\{c/4+(p''-x)/4d\}/\cdot 6020k$

or $k(\log q'-\log p')\{c/2+(p'-4x)/2d\}/\cdot 6020k$

or $k(\log q -\log p)\{c+(p-16x)/d\}/\cdot 6020k$

But

$\log q-\log p = \log (1+n/p)$ $(q = p+n)$

And $d = c/48x$ (by definition)

Hence

$PY = (1/6+p/12x)(c/\cdot 6020) \log (1+n/p)$ $x \leqslant p \leqslant 4x$

or $(1/3 \cdot p/24x)(c/\cdot 6020) \log (1+n/p)$ $4x \leqslant p \leqslant 16x$

or $(2/3+p/48x)(c/\cdot 6020) \log (1+n/p) 16x \leqslant p \leqslant 64x$

TABLE OF ISOPLUS FUNCTION IN ARBITRARY UNITS

p \ n	$\frac{1}{4}$	$\frac{1}{2}$	$\frac{3}{4}$	1	$1\frac{1}{4}$	$1\frac{1}{2}$	$1\frac{3}{4}$	2	$2\frac{1}{2}$	3	$3\frac{1}{2}$	4	$4\frac{1}{2}$	5	6	7	8	9	10	11	12
1	4·0	7·3	10·1	12·5	14·6	16·5	18·2	19·8	22·6	25·0	27·1	29·0	30·7	32·3	35·1	37·5	39·6	41·5	43·2	44·8	46·3
1·5	3·2	6·0	8·5	10·8	12·8	14·6	16·3	17·8	20·6	23·1	25·3	27·3	29·2	30·9	33·9	36·5	38·8	40·9	42·9	44·6	46·2
2	2·8	5·4	7·7	9·8	11·7	13·5	15·1	16·7	19·5	22·0	24·3	26·4	28·3	30·1	33·3	36·2	38·7	41·0	43·1	45·0	46·8
2·5	2·6	4·9	7·1	9·1	11·0	12·7	14·4	15·9	18·8	21·3	23·7	25·9	27·9	29·7	33·1	36·1	38·8	41·3	43·5	45·6	47·6
3	2·4	4·6	6·7	8·6	10·5	12·2	13·8	15·4	18·2	20·8	23·2	25·5	27·5	29·5	33·0	36·2	39·1	41·7	44·1	46·3	48·4
3·25	2·3	4·5	6·6	8·5	10·3	12·0	13·6	15·1	18·0	20·6	23·1	25·3	27·4	29·4	33·0	36·3	39·2	41·9	44·4	46·7	48·8
3·5	2·3	4·4	6·4	8·3	10·1	11·8	13·4	14·9	17·8	20·5	22·9	25·2	27·3	29·3	33·0	36·3	39·3	42·1	44·6	47·0	49·2
3·75	2·2	4·3	6·3	8·2	9·9	11·6	13·2	14·8	17·7	20·3	22·8	25·1	27·3	29·3	33·0	36·4	39·5	42·3	44·9	47·3	49·6
4	2·2	4·3	6·2	8·0	9·8	11·5	13·1	14·6	17·5	20·2	22·7	25·0	27·2	29·3	33·0	36·5	39·6	42·5	45·2	47·7	50·0
4·25	2·1	4·1	6·0	7·8	9·5	11·1	12·7	14·2	17·0	19·6	22·1	24·4	26·6	28·6	32·4	35·8	39·0	41·9	44·5	47·0	49·4
4·5	2·0	4·0	5·8	7·5	9·2	10·8	12·3	13·8	16·6	19·2	21·6	23·9	26·0	28·1	31·8	35·3	38·4	41·3	44·0	46·5	48·8
4·75	2·0	3·8	5·6	7·3	8·9	10·5	12·0	13·5	16·2	18·8	21·4	23·4	25·5	27·6	31·3	34·7	37·8	40·7	43·4	45·9	48·3
5	1·9	3·7	5·5	7·1	8·7	10·2	11·7	13·1	15·8	18·4	20·7	23·0	25·1	27·1	30·8	34·2	37·3	40·2	42·9	45·5	47·8
6	1·7	3·4	5·0	6·5	8·0	9·4	10·8	12·1	14·7	17·1	19·3	21·5	23·5	25·5	29·2	32·5	35·6	38·6	41·3	43·8	46·2
7	1·6	3·1	4·6	6·0	7·4	8·7	10·1	11·3	13·8	16·1	18·3	20·4	22·4	24·3	27·9	31·3	34·4	37·3	40·0	42·6	45·0
8	1·5	2·9	4·3	5·7	7·0	8·3	9·5	10·7	13·1	15·3	17·5	19·5	21·5	23·4	26·9	30·2	33·3	36·3	39·0	41·6	44·1
9	1·4	2·8	4·1	5·4	6·6	7·9	9·1	10·2	12·5	14·7	16·8	18·8	20·7	22·6	26·1	29·4	32·5	35·4	38·2	40·8	43·3
10	1·3	2·6	3·9	5·2	6·4	7·6	8·7	9·9	12·1	14·2	16·2	18·2	20·1	21·9	25·4	28·7	31·8	34·7	37·5	40·1	42·7
11	1·3	2·5	3·8	5·0	6·2	7·3	8·4	9·5	11·7	13·8	15·8	17·7	19·6	21·4	24·8	28·1	31·2	34·1	36·9	39·6	42·1
12	1·2	2·5	3·7	4·8	6·0	7·1	8·2	9·3	11·4	13·4	15·4	17·3	19·1	21·0	24·4	27·6	30·7	33·6	36·4	39·1	41·7
13	1·2	2·4	3·6	4·7	5·8	6·9	8·0	9·0	11·1	13·1	15·0	16·9	18·8	20·6	24·0	27·2	30·3	33·2	36·0	38·7	41·3
14	1·2	2·3	3·5	4·5	5·6	6·7	7·8	8·8	10·9	12·8	14·8	16·6	18·4	20·2	23·6	26·8	29·9	32·8	35·6	38·4	40·9
15	1·2	2·2	3·4	4·5	5·5	6·6	7·6	8·6	10·7	12·6	14·5	16·4	18·1	19·9	23·3	26·5	29·5	32·5	35·3	38·0	40·6
15·25	1·1	2·3	3·3	4·5	5·5	6·5	7·6	8·6	10·6	12·6	14·5	16·3	18·1	19·8	23·2	26·4	29·5	32·4	35·3	37·9	40·6
15·5	1·1	2·2	3·3	4·5	5·5	6·5	7·6	8·6	10·5	12·5	14·4	16·2	18·0	19·8	23·1	26·3	29·4	32·4	35·2	37·9	40·5
15·75	1·1	2·2	3·3	4·4	5·4	6·5	7·5	8·5	10·5	12·4	14·3	16·2	18·0	19·7	23·0	26·2	29·3	32·3	35·1	37·8	40·4
16	1·1	2·2	3·3	4·4	5·4	6·5	7·5	8·5	10·5	12·4	14·3	16·1	17·9	19·6	23·0	26·2	29·3	32·2	35·0	37·8	40·4
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16·5	1·1	2·1	3·2	4·3	5·3	6·3	7·4	8·3	10·3	12·2	14·0	15·8	17·6	19·3	22·6	25·8	28·8	31·7	34·5	37·2	39·8
16·75	1·1	2·1	3·2	4·3	5·3	6·3	7·3	8·2	10·2	12·1	13·9	15·7	17·5	19·2	22·4	25·6	28·6	31·5	34·3	37·0	39·6
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18	1·0	2·1	3·1	4·1	5·0	6·0	7·0	7·9	9·8	11·6	13·3	15·1	16·8	18·4	21·6	24·7	27·6	30·5	33·2	35·8	38·4
19	1·0	2·0	2·9	4·0	4·9	5·8	6·7	7·6	9·5	11·2	13·0	14·7	16·3	17·9	21·0	24·0	26·9	29·7	32·4	35·0	37·5
20	1·0	1·9	2·9	3·8	4·8	5·7	6·6	7·5	9·2	10·9	12·6	14·3	15·9	17·4	20·5	23·4	26·3	29·0	31·7	34·2	36·7
22	·9	1·9	2·7	3·6	4·5	5·3	6·2	7·1	8·8	10·4	12·0	13·6	15·1	16·6	19·6	22·1	25·2	27·8	30·4	32·9	35·3
24	·8	1·7	2·6	3·5	4·3	5·1	5·9	6·7	8·3	9·9	11·5	13·0	14·5	15·9	18·8	21·6	24·2	26·8	29·3	31·7	34·1
26	·8	1·6	2·5	3·3	4·1	4·9	5·7	6·5	8·0	9·5	11·0	12·5	13·9	15·3	18·1	20·8	23·4	25·9	28·4	30·8	33·1
28	·8	1·6	2·4	3·2	4·0	4·7	5·5	6·2	7·7	9·2	10·6	12·0	13·5	14·8	17·5	20·1	22·7	25·1	27·5	29·9	32·2
30	·8	1·6	2·3	3·0	3·8	4·5	5·3	6·1	7·4	8·9	10·3	11·6	13·0	14·4	17·0	19·5	22·0	24·4	26·8	29·1	31·3
32	·8	1·5	2·2	2·9	3·7	4·4	5·2	5·9	7·2	8·6	9·9	11·3	12·7	14·0	16·6	19·0	21·5	23·8	26·2	28·4	30·6
34	·7	1·5	2·2	2·8	3·6	4·3	4·9	5·7	7·0	8·4	9·7	11·1	12·3	13·6	16·1	18·6	20·9	23·3	25·6	27·8	30·0
36	·7	1·4	2·1	2·8	3·5	4·2	4·9	5·6	6·8	8·1	9·5	10·8	12·0	13·3	15·8	18·1	20·5	22·8	25·1	27·3	29·4
38	·7	1·4	2·1	2·7	3·4	4·0	4·7	5·4	6·7	8·0	9·3	10·4	11·7	13·0	15·4	17·8	20·1	22·4	24·6	26·7	28·9
40	·6	1·4	2·0	2·7	3·3	4·0	4·7	5·3	6·6	7·8	9·1	10·3	11·6	12·8	15·1	17·4	19·7	22·0	24·1	26·3	28·4
45	·6	1·3	1·9	2·5	3·2	3·8	4·4	5·0	6·3	7·5	8·7	9·9	11·0	12·2	14·4	16·8	18·9	21·1	23·2	25·3	27·4
50	·6	1·2	1·8	2·4	3·0	3·6	4·2	4·8	6·0	7·2	8·3	9·5	10·6	11·7	14·0	16·1	18·3	20·4	22·5	24·5	26·5
55	·6	1·2	1·8	2·3	3·0	3·5	4·1	4·6	5·8	7·0	8·1	9·2	10·3	11·4	13·5	15·6	17·7	19·9	—	—	—
60	·5	1·1	1·8	2·3	2·9	3·4	3·9	4·5	5·7	6·7	7·8	9·0	—	—	—	—	—	—	—	—	—

$PY=(\cdot13843\ c/mx)\ (2m^3x+p)\log(1+n/p)$ (1 ($1\leq p\leq 4$)

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Hence

$$PY = (m/6 + p/12mx)(c/\cdot 6020) \log (1 + n/p)$$
$$= \cdot 13843c/mx\ (2m^2x + p) \log (1 + n/p)$$

$$\text{when: } \begin{cases} x \leqslant p \leqslant 4x & : m = 1 \\ 4x \leqslant p \leqslant 16x & : m = 2 \\ 16x \leqslant p \leqslant 64x & : m = 4 \end{cases}$$

ACKNOWLEDGEMENTS

I am indebted to David A. Hopkins and Miss Barbara Tyas for their help in preparing this paper, and to the Management of Samuel Fox and Co. Ltd. for permission to publish it.

ABSTRACTS AND REVIEWS

Efficient Transport.

F. GERST.

Tydschrift voor Efficientie en Documentatie, 1954; **24**, 311–314.

An attempt is made to assess and analyse some aspects of inefficiency in road transport. Three "case histories" are given. The author sees as one of the main causes of inefficiency the failure of hauliers and their customers alike to understand each other's specific problems.

Operations Research: Past, Present and Future.

PHILIP M. MORSE (M.I.T.).

Advanced Management, Nov., 1954; **19** (11), 10–15.

The author shows how unrelated facts which emerge through study become related as more careful investigation is carried out. The full benefits of operations research do not come until many data are gathered and analysed in the correct way. The waiting in line or queueing problem is discussed as an example of applied research. The uses of such tools as the Monte Carlo method, linear programming and game theory are discussed. The need for the continual checking of mathematical models with experiments is stressed. Examples of wartime uses of operations research are quoted and the close co-operation between research and management which is so essential if things are to go smoothly, made clear.

A Statistical Investigation into Factors Affecting the Life of Ladle Linings.

J. E. ANDREW (United Steel Cos.).

Trans. Brit. Ceramic Soc., Oct., 1954; **53** (10), 609–620.

The paper deals principally with the effect of several steelmaking variables on lining life. Two methods of analysis are available, the more exact involving an assessment of the brick wear caused by each cast during a life. Section 1 of the statistical analysis of data is mainly concerned with estimating the accuracy of the dip-stick reading which, together with the ingot weight, can be used to measure the maximum capacity of the ladle for each cast, and hence the wear as life proceeds.

It is shown that the dip-stick measurement errors were too high for the detailed analysis proposed. Further work revealed that the capacity of the ladles, even when first put into service, was about 11 steel tons in excess of the average weight of steel held. Estimates of wear showed that, at the end of a lining life, about one-third of the brickwork had been lost.

Section 2 of the statistical analysis shows that the bricks from different makers were not equally good, and that the shape of the ladle appeared to affect life. The effects upon life of the percentage of carbon and of tap iron, the time from tap to finish teem and the slag depth were determined, and the difference in life between ladle shapes was accounted for by the different slag depths carried.

As a result of the analysis, several recommendations can be put forward for improving lining life.

The Scientific Basis for Decisions (Operational Research).

R. DE CHANTAL (Bureau des Temps Elémentaires).

L'Etude du Travail, Feb., 1955; 3–13 (*in French*).

The author gives a detailed account of the operational research seminar organized by Professor T. U. Matthew and held at the Institute for Engineering Production of Birmingham University in April 1954. The aims of operational research are described and the rapid development of the service since the days of World War II is reviewed. Research methods are discussed in some detail and the mathematical principles underlying these methods are outlined. Mention is made of the queue theory, sequential analysis, linear programming and cybernetics. The examples described include cases where the solution is to be found in the best compromise

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between a number of conflicting interests such as exist in production, queueing and marshalling problems. Finally, the author discusses the means of establishing operational research departments in industry and the best way of recruiting suitable staff.

The Scientific Basis for Decisions (Examples).

R. De Chantal (Bureau des Temps Elémentaires).

Travail et Méthodes, Dec., 1954; 3–7 (*in French*).

The author shows how by mathematical methods and operational research it is possible to determine the best course of action to take when confronted with problems depending on random events.

The examples, which are worked out in some detail, deal with inventory control and queueing problems.

Report of Conference on Works Transport.

British Iron and Steel Research Association, London, 1954.

This report gives the full text of the papers (with discussions) read at the 1954 conference on Works Transport which was organized jointly by the Plant Engineering Division and Operational Research Section of the British Iron and Steel Research Association. The ten papers given were grouped under four main headings. These dealt with relations between British Railways and the industry; managerial problems; the O.R. approach to the solution of traffic problems; organizational aspects of running and maintaining rail and road services, steam and diesel traction and wagon design.

The session devoted to operational research dealt with traffic congestion problems and the economics of works transport. Congestion, it was maintained, occurred because the storage facilities which were provided to even out discrepancies between arrival and consumption rates of materials were either too small or inefficiently used. A method of fixing the best size of siding capacity was put forward, based on the economic balance of having large sidings on the one hand, holding materials under load and paying demurrage, and on the other, having smaller sidings and discharging materials to ground, thus avoiding demurrage charges but paying for the double handling of the commodities.

The question of locomotive running costs was dealt with in some detail and it was suggested that the wide differences between costs in different works could not be accounted for by working conditions

alone but must be ascribed to lack of consistency in costing techniques. Standard costing was recommended as it was considered more important to know by how much and why actual costs diverged from standard than merely to know at what level costs stood. The effect of locomotive utilization on costs was discussed and it was shown that with the poor level of utilization achieved in some works, running costs for diesel and steamers could be as high as 45s. and 60s. respectively per effective locomotive hour.

Operations Research for Management.

Edited by JOSEPH F. MCCLOSKEY and FLORENCE N. TREFETHEN.

The Johns Hopkins Press. Baltimore, 1954; xxiv+409 *pp.* $7.50 (*Distributed in Great Britain by Geoffrey Cumberlege, Oxford University Press, London.* 60s. nett.)

During the spring of 1952, the Johns Hopkins University decided to sponsor an informal seminar on operations research. In this book are published some twenty-three papers by different authors presented to the seminar during 1952–53.

The papers are grouped together in three parts: the first is general (history, O.R. as a profession, O.R. as a science, organization, and management consulting); the second is methodology (statistics in O.R., queue theory, information theory, suboptimization, symbolic logic, computing machines, linear programming, and game theory) and includes other brief introductions to the subjects with, in most cases, examples of their application; and the third contains 8 papers on case histories. There are also a 19-page bibliography, with references to some 350 published papers, and an index.

If, as is stated in the foreword, the idea of publishing these papers as a comprehensive introduction to the subject, only came later, the organiser of the seminar must be congratulated on his serendipity; for the editors have been able to fit the pieces together into a coherent book which tells an impressive story. The papers were presented individually in a university atmosphere and it is not surprising, therefore, that many of them include rather more technical matter than would be expected in a book intended for management. The presentation, however, maintains a high standard of clarity and mathematics do not intrude to a degree that would be bothersome to the non-technical reader. The editors have done well to leave the papers as they stand and to present a well-balanced picture of the subject which can be recommended for both management and technical readers.

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OPERATIONAL RESEARCH SOCIETY

Programme for the Session

The following meetings have been arranged for the 1955/56 season. As the rooms of the Royal Society are not this year available, owing to taxation difficulties, meetings will be held in the Council Room of the Agricultural Research Council, 15 Regent Street, London.

13 October: H. G. Jones of the Steel Company of Wales, *The Ultimate Capacity of the Slabbing Mill.*

25 November: Miss S. V. Cunliffe of Messrs. Guinness, *Some Experiments on Growing and Picking Hops.*

16 December: N. Jessop of Courtaulds, on a subject to be announced.

January: The Earl of Halsbury, *The Work of the National Research Development Corporation.*

February: Dr. H. F. Rance of Wiggins, Teape & Co., *Objective and Subjective Factors in the Control of the Quality of Paper.*

March: Conference on *the Recording and Processing of Data in Operational Research.*

The annual general meeting will precede the meeting on 13 October.

ISOPLUS n

Interpolating an Additive Cycle in a Multiplicative Multistage Nomograph

The author writes:

This note (*a*) corrects a mistake which occurs in the above paper,* (*b*) elaborates a statement connected with this point, and (*c*) replies to a criticism.

(*a*) In paragraph (iv) of "The Proof of Isoplus Zero" (page 79), it is stated that "the projected scalar intervals on EM are all equal", and that "both XE and ZZ are internally divided into equal intervals". This should have said that the intervals are *virtually* equal: we are using a close approximation. The effect of this approximation can be calculated mathematically where it is felt on PY, and the influence of this vertical discrepancy can in turn be measured in the angle at O which subtends PY. The calculations show that the approximation, at its crudest, is unlikely to result in an error there greater than 10 minutes of arc. Since the user has to align through O, a degree of experimental error at this point considerably greater than that due to the mathematical approximation must be expected. Hence this note has no practical relevance for the nomographer who wishes to use the isoplus function and tables; its object is to modify a statement which has a false appearance of rigour.

(*b*) On page 76 I said, without explanation, that the "original scale's geometric intervals will have to be transformed to arithmetic intervals". These are the virtually equal intervals mentioned above. It is of course *possible* to derive a family of isoplus curves based on an arbitrary isoplus zero, and I should have made this clear. The point I wanted, and remain anxious, to make is that this process is of very little practical value in terms of accuracy, if the projected intervals are *not* roughly equal. The isoplus zero system of three straight lines, each having (virtually) even calibrations, is suggested as one of the simplest and most useful ways of handling a graphical situation which could otherwise be quite grossly inaccurate.

(*c*) It is for cognate reasons that calculated PY values have been provided to supply the locus of isoplus n. These will define a curve in which the nomographer can feel confident, within the fully adequate standard of approximation implied by (*a*). It has been suggested to me that it might be better to construct isoplus n empirically. Better draughtsmen than I may agree with the judgment of these correspondents, and the issue will be decided as a matter

* Beer, Stafford. *Operat. Res. Quart., June* 1955, **6** (2), 74.

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of preference. There is however this note of warning: I evolved the expression and calculated the tables for isoplus n for the very reason that the original nomograph incorporating this additive cycle, which had been constructed empirically, proved on test to be seriously inaccurate.

SCALE FACTORS IN THE MANAGEMENT OF COAL MINES

by

R. W. REVANS*

IN THE SPRING OF 1951 the writer went to live with a miner's family in a small, self-contained coalfield. During his stay there he detected (or thought he detected) a marked difference between the attitudes towards the National Coal Board of the miners employed in small pits, and of those employed in large ones. The evidence for this was not entirely subjective; there were significant differences in the attitudes of the men towards the entry of their sons into the industry, in their knowledge of the policies of the Coal Board, and of its leading figures, both local and national, and of their interest in such matters as consultation and the Coal Board's productivity campaigns. Without pretending that this itself, although statistically significant, demonstrated the existence of some social factor that might be called morale, and that was dependent upon the size of the colliery, it was nevertheless thought worth while to investigate how far the size of the colliery in which a man worked might influence his behaviour.

Attendance

Figure 1 shows for all the mines in the coalfield in which the writer stayed, the relation between the number of men on books and the average number of bonus shifts earned per man during the first quarter of 1951. Since a bonus shift is earned in every week that a miner works every day from Monday to Friday inclusive, the average number of bonus shifts earned is a crude measure of attendance.

It will be noticed that there is a marked tendency for the men in the smaller pits to earn more bonus shifts than those in the larger; the marked exception turned out, on investigation, to be a pit in extreme geological difficulties. It was closed shortly after these records were collected.

The statistics for all mines in the country for the year 1942 also show a marked size factor. This is demonstrated by Table I, which

* Professor of Industrial Administration, University of Manchester.

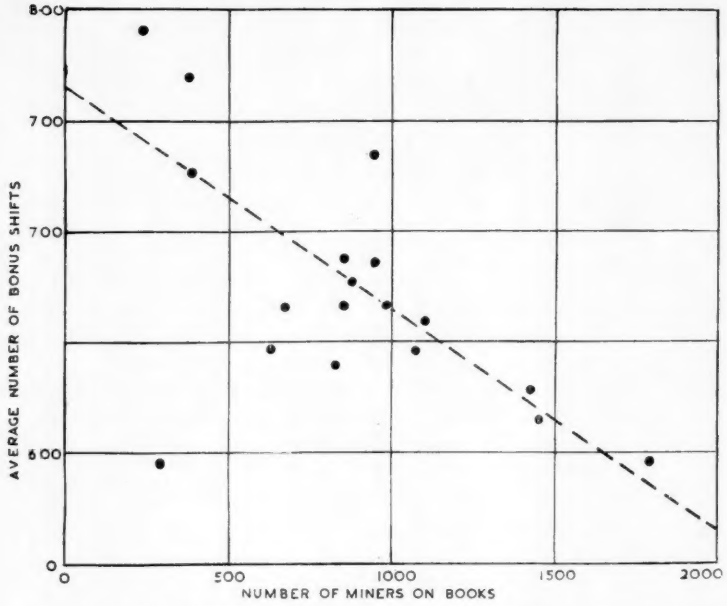

FIGURE 1. *Size and attendance at* 18 *pits constituting an N.C.B. Area; Spring Quarter,* 1951.

TABLE I

ABSENCE IN SMALL AND LARGE MINES, 1942

Coalfield	Number of Mines		Average Absenteeism		Joint Standard Deviation
	Smaller	Larger	Smaller per cent	Larger per cent	
Scotland	108	12	5·65	7·50	3·247
Northern	92	46	6·72	8·41	3·343
Yorkshire	42	56	10·02	13·73	3·071
North Midland	27	35	9·05	14·19	3·326
Midland	61	20	7·71	13·45	3·960
Lancashire and North Wales	41	21	7·43	13·12	3·649
South Wales	79	29	7·51	10·98	4·239
Great Britain	450	218	7·15	11·66	3·828

(Source: Ministry of Fuel and Power)

gives, for the major coalfields of Britain, the mean absenteeisms in all mines employing less than 100 men and in all mines employing over 1,000 men, together with the standard deviations. The tendency for the attendance to be worse in the large pits is significant beyond

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all doubt. A not entirely invalid conclusion from Table I would be that at least four percentage points of the absenteeism of the larger pits may be attributed to size or social factors, so that the purely personal non-attendance of the miners (making due allowance for normal absence due to sickness and accidents) was, at least in 1942, a small if not negligible, quantity.

It should be remembered that in 1942 the coal industry was not nationalized.

Accident Rates

Table II shows the average tonnage lost in disputes per person size groups, the mean compensable accident rate per 100,000 manshifts in 1946, and in 1950. This shows that the risk of any individual miner's having an accident is, averaged over size groups of mines, proportioned to the logarithm of the number of men in the size group. The whole weight of this evidence, drawn from over 300 million manshifts, suggests that a miner working in a mine where

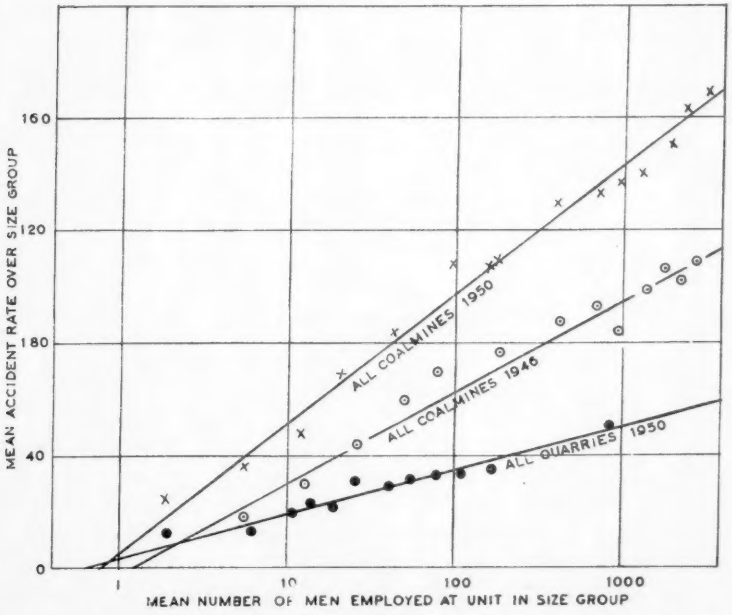

FIGURE 2. *Relation between mean accident rate and log. mean size of all coal mines,* 1946 *and* 1950, *and of all quarries,* 1950.

he was the sole employee would never have an accident. This result should suggest to all, if it does not justify to some, the existence of social factors influencing morale.

It might be suggested that the big mines are simply deeper or hotter or more mechanized and that the effect is not a true size effect, but an environmental one. Figure 2, therefore, adds the accident rate in 4,000 British quarries classified by size groups, and this shows the same general result. Quarries have no environmental differences comparable to those between shallow and deep pits. In any case, methods of partial correlation may be used to show that factors, such as mechanization and depth, still leave the scale factor as overwhelmingly significant in determining the compensable accident rate. The evidence available to the writer, indeed, suggests that mechanization tends to decrease the overall risks of accidents, or rather that the collieries that were managed by companies who were willing to instal machinery were also the collieries with low risk of accident.

Tonnage Lost in Disputes

Table II shows the average tonnage lost in disputes per person employed at all mines in two large coalfields; the first, Yorkshire, employs about 135,000 men; the second, the bituminous coalfield of South Wales, employs about 80,000 men. The figures refer to the five years 1949 to 1953 and include tonnages lost in "go slows" as well as in actual stoppages. These figures show very clearly that there is a significantly greater tendency for men to join or persist in disputes in large mines than in small. The difference between the losses in the various size groups cannot possibly be explained by differences in output per manshift since this hardly changes throughout the size range.

TABLE II

TONS LOST PER MAN IN DISPUTES, 1949–53

Men on Books	South Wales		Yorks	
	No. of Pits	Tons Lost Per Man	No. of Pits	Tons Lost Per Man
0– 500	35	9·7	28	5·9
500–1,000	40	12·1	25	9·0
1,000–1,500	23	15·0	20	16·4
1,500–2,000	6	23·4	21	12·8
2,000–2,500	—	—	8	16·0
2,500 and above	—	—	12	22·7

A more detailed analysis of these figures divides the stoppages or go-slows into two classes according to whether they are, in the eyes of the management, caused by wages issues or not. It shows that in Yorkshire where the mines are, judged by English standards, very large, the size effect is due almost entirely to non-wages issues.

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In other words, although miners in the very large pits in Yorkshire are engaged in disputes much more frequently than those in the medium and smaller pits of the same coalfield, in these larger pits the causes of the extra disputes are almost entirely such things as working conditions, colliery organization, and other non-monetary causes.

Output per Man-year

Table III is taken from the Annual Statistical Digests of the Ministry of Fuel and Power and shows the output per man-year of all collieries by size groups in Britain between 1947 and 1952. It is difficult to read anything conclusive from these figures because they are composed of pits of every possible age, depth, seam conditions and so forth. The figures do, however, suggest that if the very large mines have advantages in terms of productivity over the smaller ones we have yet to discover by what means these advantages may be secured.

TABLE III

OUTPUT PER MAN-YEAR BY SIZE OF MINE

Size	Tons					
	1947	1948	1949	1950	1951	1952
250– 499	242·7	242·8	258·3	265·8	272·6	271·5
500– 749	255·9	258·4	275·4	282·7	294·3	286·2
750– 999	252·3	266·8	273·4	289·8	294·2	295·6
1,000–1,499	280·1	283·2	298·5	313·7	324·7	316·1
1,500–1,999	263·6	285·6	307·0	321·8	325·6	323·8
2,000–2,499	265·3	277·1	286·3	301·8	306·7	309·4
2,500–2,999	261·5	291·3	295·6	302·6	310·4	294·9
3,000 and above	288·4	243·6	270·9	280·7	272·8	282·4
All	262·2	272·3	286·4	298·4	306·7	303·0

Need for Knowledge of Size Effect

There are valid technical reasons that make it necessary for the industry to be reconstructed in terms of larger mines. But for this reconstruction policy to succeed it is desirable that the disabling factors of size should be understood. If these disabling effects are the result of causes falling within the control of the administration or management, it must be an essential policy of the industry either to remove these causes or to reduce their action as far as possible.

No doubt the full range of factors that hamper the large mine can never be known. It may be said, for example, that large mines tend to draw their men from wide catchment areas and hence that they tend to employ a greater proportion of marginal labour. The writer has not been able to find any satisfactory evidence for of against this, but his studies are admittedly incomplete. It may be said that in the small pits the men know one another better, or that, just as small towns have lower larceny rates than large ones, it is

possible in smaller mines for men to have more respect for common standards of behaviour than in large. But these are difficult fields, in which analogy is dangerous.

A number of unconnected facts have, however, suggested to the writer that one of the principal disabling causes of the large pit is the relative dilution of its management, particularly in its higher ranges. A pit employing 3,000 miners may have but one manager and one under-manager trying to do the same kind of task for 3,000 men as a manager and under-manager will do at a colliery employing only 100 men. A statistical study of 34 very large pits in the same geological coalfield shows that those in which the underground workers are all concentrated in one seam perform less satisfactorily that those in which the underground workers are employed in more than one seam. This suggested, at least to the writer, that some factor internal to the colliery and depending upon the groupings of the men influences their morale. Again, in a pair of collieries employing a total of 5,000 men, where the agent has been active in strengthening the central management by the appointment of technical assistants and extra under-managers, the morale and output of the collieries are improving in a significantly gratifying way. ("Morale" in the sense used here is defined below.) In absence from accidents alone there were 4,000 man-weeks fewer lost in 1954 than in 1950 at these two pits. This suggests that when the capacity of the central management of the pit is increased, so that it becomes more able to deal with its problems, the men respond with better attendance, fewer disputes, fewer accidents and even a greater output per man-shift. It is to test this general hypothesis—that the disabling effect of the large mine is significantly a "dilution of management" effect—that the present studies are being made. But the writer does not, of course, believe that collieries can be run more successfully merely by attending to organization. Whatever irritation may be removed from a miner's lot by improving the technical organization of his work, the management must still treat him as a human being and be ready to discuss his claims and his worries in a fair and friendly way, whatever the size or organization of the mine. In the writer's view, the perfection of the management organization offers no solutions to the troubles of coalmining merely in terms of what James Burnham has called the Managerial Revolution.

The General Concepts of Management Structure

A modern pit comprises a number of faces drawing coal up the sample shaft. A multi-shaft colliery may have two, three, or even four pits. The colliery will have between five and seventy miles of

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underground roadway and any of this roadway where men work or travel regularly must be inspected statutorily for safety twice in every shift by a "deputy". The pit is therefore divided into deputies' districts. The outlying districts will include a coal face and are called "face districts" or "coal-producing districts". The roadways nearer the pit bottom—the "outbye districts"—will not include a face. The deputy naturally supervises the work of the men in his district and becomes the first-line foreman in the pit. He is aided on the face by the shotfirer, a highly responsible and experienced workman who is responsible for firing the explosive charges on the face. The deputy is in turn responsible to an "overman". There will be several overmen in the pit, each in charge of a group of deputies' districts. The overmen are responsible to the under-manager. The overmen, deputies and shotfirers are known collectively as "under-officials".

The main managerial tasks of the colliery are two-fold: (1) to control the face operations where they are carried out, that is, on the face; this means that the coal has to be properly cut, fired and filled; that the timber has to be properly set; that the standard of packing conforms to whatever rules have been made; that there is regular maintenance of any face machinery, and so forth; (2) to provide adequate pit or seam services so that the faces are supplied with whatever they need to produce coal; such services as keeping the roadways in good repair, a reliable coal clearance system, a sufficient supply of materials, the timely replacement of machinery or parts of machinery from the colliery workshops, the provision of expert technical advice upon methods of working, and so forth.

The second type of control will involve not only the under-manager normally in charge of a seam or seams, but also other officials such as the unit engineer, the store keeper, the surface foreman in charge of dirt disposal, and so forth. It is the main business of the manager when he is acting as chief of operations to see that his under-manager is adequately provided with the services of which these other senior officials are in charge.

The Effect of Size on Control

If the pits are ranked in order of size two decisive trends stand out: the first is that the average number of men under the deputy increases; the second that the total number of deputies to be co-ordinated by the under-manager also increases. Larger pits, therefore, have severer problems of control than smaller ones *at both*

levels of management; the under-manager not only has more faces to co-ordinate, but the face-group tasks themselves make more demand upon the deputies.

The evidence for these effects can be found in a number of ways. In order to eliminate differences between coalfields it is convenient to treat the 47 administrative areas, into which the National Coal Board divided the industry in 1949, as separate units. In each of these units the total number of pits is divided equally, the larger pits falling into one group and the smaller into the other. For each of the 94 groups forming the 47 pairs the average number of colliers per under-official is worked out, together with the 47 differences-within-pairs. If there were no "size effect" the mean of these 47 differences would not be significantly different from zero. It is, in fact, highly significantly positive, showing that irrespective of Area conditions (and hence of seam conditions) pits with more men in total have more colliers per under-official.

The second size trend is shown in Table IV; this gives the number of under-managers in all N.C.B. mines classified by total tonnage drawn during 1953. It can be seen that although large mines tend to have more than one under-manager the total number

TABLE IV

SIZE OF MINE AND NUMBER OF UNDER-MANAGERS
(SIZE BY TONNAGE DRAWN IN 1953)

	Below 100,000	100,000–200,000	200,000–350,000	350,000–700,000	Above 700,000
No. of Mines	250	178	190	167	45
No. of U/Ms	257	198	253	274	98
Average U/Ms per mine	1·03	1·11	1·33	1·63	2·18
Average Tons per U/M	63,000	135,000	206,000	320,000	370,000

of under-managers does not grow as rapidly as the output of the mines themselves. There is, in other words, no tendency for the size of the under-manager's task in the big mine to be offset by the appointment of more under-managers. Nor is this all. There is no record, known to the writer, of any single-seam mine, however large, employing more than one statutory under-manager; a mine drawing 25,000 tons of coal per week from one seam (or two seams so close together as to be run as one) channels as much operational responsibility through one under-manager as do many mines drawing only a tenth of this weekly tonnage from one seam. The writer is not aware of any *systematic* attempt to give the under-managers of the larger pits any additional resources to carry out their heavier and more complex tasks, though individuals here and there may have secured some.

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Geological Conditions, Structure and Morale

It is well known that large mines tend to work thick seams. This may be because very deep mines are not normally sunk to work thin measures, since they do not repay the heavy cost of sinking unless they can draw a large output—which they can only do by employing large numbers of men. Moreover, there is a general tendency for thick seams to be laid out by mining engineers so as to work big coal face districts. There seems to be a temptation to limit the size of faces in thick seams by the capacity of the face conveyors to get the coal away. Hence, it might be concluded that the "size effect" is basically a thick seam effect. This is the argument which says that more accidents occur in big mines because they work thick seams, and thick seams are believed to be inherently more dangerous; it is also believed that the greater freedom of movement of men along the face in a thick seam leads to the more rapid spread of rumour and disaffection. It is in fact asserted by experienced managers that miners in thin seams, who cannot readily talk to one another, work much more contentedly.

The evidence of the previous section suggests that even in similar seam conditions, that is, within the same administrative Area, there is a tendency for the mine employing the more men to have the bigger face districts. But the effect can be demonstrated in another way. If for the 115 pits in Yorkshire a correlation analysis is made using:

(i) M, total number of men employed at the pit;
(ii) S, number of deputies per 1000 underground workers; and
(iii) T, mean seam thickness;

one finds the partial correlation between S and M, holding T constant, to be −0·432. If there were, therefore, no variation between the thickness of the seams in Yorkshire there would still be a highly significant inverse correlation between S, the intensity of the supervision and M, the total number of men in the pit. But since S is inversely proportional to the size of the face group, this in turn is significantly and positively correlated with M, the total size.

This result may be taken to suggest that there is generally throughout the industry a desire—or even a policy—to work the maximum area of coal with the minimum number of officials. More men are accommodated in large mines not so much by opening more faces, as by raising the average number of men on the faces.

It is then possible (although this anticipates the next sections of this paper) to ask how far the general morale of the 115 miners

is correlated with the thickness of the seams, on the assumption that both the overall size and the intensity of face-group supervision were held constant. General morale can be defined by the tonnage of coal lost per man in accidents and disputes—the two indices that are apparently least susceptible to the volition of the men. Unlike absenteeism, which is largely a result of conscious decision by the individual, disputes are collective phenomena and accidents, insofar as they contain an element of volition, are motivated below the level of consciousness. The sum of the coal lost per man from compensable accidents and strikes over a period of five years, here called L, was estimated for all the Yorkshire mines; it was then found that the partial correlation for the whole coalfield of L and T, holding M and S constant, was only +0·042, which is not significant. Hence, given uniform structure conditions, that is assuming total size and face group size to be constant, there is no relation between morale and seam thickness. This suggests that accidents are caused, and disputes encouraged, not by seam thickness but by the social, or management, structure which is built into pits with thick seams.

Proposed Study of Representative Pits

To find out how much the relatively poor morale of the large pit is bound up with the features of its structure, a study has been started of 24 representative pits drawn from seven different coalfields. The inquiry is being conducted under the aegis of a panel of colliery managers nominated by the National Association of Colliery Managers. The data sought include information about colliery plans, disposition and horse-power of machinery, location of telephones, meeting stations, and so forth. The structure of the management from the deputy up through the overman and undermanager is being traced from the coal face outwards; an attempt is also being made to study the system for controlling the haulages, for getting in supplies of materials and for maintaining and repairing in emergency the operating machinery. Finally, an attempt is being made to measure the flow of information throughout the collieries by a study of their reporting systems.

For each coal-producing district (including either a long wall face or a group of bord and pillar workings) an attempt has been made to estimate two quantities:

(i) the supervisory load internal to the district and falling upon the deputy in charge; this will depend on the number of face workers, the length of the face, the total tonnage drawn

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from it, the length of the roadways leading into it, and so forth. (The measures of these different quantities are, of course, non-comparable, although highly correlated);

(ii) the strength and flexibility of the services made available to the coal faces by the outbye (or central) management; these will be suggested by the length, complexity and condition of the haulage system; by the system for getting in supplies of materials; by the amount of time consumed by fitters or electricians needing to visit the districts, either for routine maintenance or in emergency; by the time available for higher officials to inspect the district and to learn what is going on in it; and so forth.

The results of this study show an enormous range in both the load carried by the officials in charge of the districts and in the complexity of the co-ordinating task carried out by senior officials.

Table V gives two faces selected from each of four pits. Various dimensions of the face are given, each of which may be regarded as a measure of the load on the deputy, the supervisor of the face.

TABLE V

RANGE OF INDICES OF DEPUTY'S SUPERVISORY LOAD

Pit	District	Yards of Roadway to be Travelled	Length of Face (Yds.)	Tons of Coal got per Shift	No. of Men in District	No. of Fillers
A	1	2,700	129	300	47	20
	2	750	146	300	37	23
B	1	1,350	207	240	29	20
	2	400	400	250	22	15
C	1	2,150	150	334	94	62
	2	5,300	220	195	65	38
D	1*	3,400	278	475	112	60
	2	3,250	232	500	121	77

* D.1 has two deputies.

Note: The deputy is required under the Coal Mines Act to walk the yards of roadway to be travelled twice in his shift.

Table VI shows some examples of the supervisory load placed on the pit under-managers. The two measures selected are the weight of coal raised from the pit each week and the number of overmen to be co-ordinated. The overman is the supervisor of a group of deputies' districts and is placed in the line of command between the under-manager and the deputy.

TABLE VI

SUPERVISORY LOAD PLACED ON UNDER-MANAGERS

Pit	No. of Under-Managers	Tons per Week	No. of Overmen
E	2	16,000	10
F	1	26,000	17
G	3	19,000	8
H	1	7,000	4

Meaning of Supervision

These few illustrations show the very large range of responsibility carried by the deputies in charge of the coal face districts. On comparable face activities, e.g. filling off the coal, one deputy may have charge of a hundred men and of over two miles of roadway, another may have only twenty men and a quarter of a mile of roadway. This suggests that whatever supervision may happen to be, the one has a much greater task than the other, and as a result may not be so effective a member of the management team. An analysis of the 830 mines over the industry as a whole shows that, if they are divided into the five size groups of Table IV, and then each one of these size groups further divided according to whether the supervision of the mines in them was high or low, the morale in every case was better in the sub-group with high supervision. High supervision meant having eight or more junior officials per 100 workers underground; low supervision meant having less than eight. Junior officials meant overmen, deputies, and shotfirers, but the total ratio is still an inverse measure of working group size. In all 15 cases—using 3 morale indices, absence, accidents, and disputes, in each of 5 size groups—the more highly supervised members of the size group responded better than the more loosely. The result is very highly significant and is not a seam thickness effect.

It can only be through careful study of the actual tasks carried out by the supervisors that the mechanism of this effect will be discovered—that is, if the assumption is true that the size of the territory and the number of miners under the charge of the face official in some direct way affects the attendance, accident rate, and strike proneness of the mine. There is nothing new in this as a matter of principle; it is merely the extension of the ideas of work study into the field of supervision. But in practice it may prove difficult to define clearly what should be the immediate responsibilities of the supervisor for the visible tasks in front of him, and what should be his responsibilities for keeping in touch with, and

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acting upon the directions of, the members of the management who are nominally his superiors. The writer feels that a new branch of work study, which might be called organization study, will need to develop in order to find out whether the management teams are most economically deployed; and this is a matter, not only of studying patterns of organization, but of the actual tasks of individuals occupying positions in those patterns. A number of such studies, with the active interest of the trade union representing the junior officials, have already been made of the deputies in charge of coal face groups.

The Supervision of the Supervisors

The deputy in charge of the working group at or near the coal face must also rely upon his immediate supervisors for the smooth running of his district. These supervisors, in turn, must be satisfied that the deputy is carrying out his tasks to the best of his ability, or certainly above the standard at which the organization of the district begins to break down. If the supervisory task of the deputy tends to be excessive the higher management of the colliery may tend to reinforce him by the provision of more second-line supervisors, that is, overmen. The overmen are of several different occupational types; with local commands to supervise the adjacent districts of several deputies; or with special responsibility for an operational function, such as the standards of roof control; or with some service function such as the control of the haulages or supplies, including the maintenance of the roadways along which these run. There seems to be a tendency to appoint more overmen in collieries where the tasks of the deputies are heavy; in other words, it looks as if the supervisory troubles of the first-line official with the large district are supposed to be countered by increasing the supervision exercised over him. But, using the methods of correlation once more over the whole of the Yorkshire coalfield, it may be shown that morale tends to be bad in collieries that, other things being equal, have *close* supervision of the deputies by the overmen. If a new index, V, given by the number of overmen per 100 deputies (and thus a positive measure of the intensity of supervision by the second line over the first) is constructed, it is found that the correlation between L, the so-called general morale index, and V, holding both S and M constant, is significant and *positive*. This is in marked contrast to the trend of L with either S or M, other things again being equal. It means this: that when either the central management of the colliery, or the average deputy working in it, has a large number of

men to look after, the morale will tend to be poor; but that when the second line, or middle management, has comparatively few deputies to look after, the morale does not tend to get better, but to get still worse. It is easy to jump to conclusions, no doubt, from such a result, but, again, only a careful study of the actual duties of the overmen and their relations to the deputies (and undermanagers) can throw any light upon the linkages between the various levels of supervision, and the extent to which the necessary functions of management are partitioned and articulated between them. A study of these affairs may even throw a great deal of light upon what "the necessary functions of management" may turn out to be. It would be unwise to assume in advance that they should conform to some set of transcendental principles.

The Concepts of Management Structure

The results of this work may appear to be no more than measured statements of the obvious. But they appear to the writer to suggest the following hypothesis: that morale, on the one hand, and the effectiveness of the management organization, on the other, are, by some psychological chain about which we know little enough at present, significantly correlated; and that insofar as the management organization is able to provide the miners with whatever it is that miners need to do their work, the morale will be good. There are two concepts which seem to the writer to be useful in developing the study further: the first is of management *opacity*, or the difficulty (inherent in the structure) that the central management experiences in getting to know the problems that continuously arise at the point of operation; the second is of the *expectation and experience* of the men who are being managed. The opacity of a structure will depend upon the number of links through which information (upwards) or instructions (downwards) need to pass, and upon the ease with which they pass from one link to the next above or below.* The ease with which the information or the instructions are transferred depends in turn upon the total supervisory load carried by each link; where these loads are high the transfer will be reluctant and spasmodic, simply because the supervisor is so driven to concentrate on his immediate face-to-face problems that he cannot properly keep in touch with his superiors. Hence an organization with many layers and each layer heavily

* The organization can, of course, also be opaque laterally, and is likely to be so if it is opaque vertically. Two men on the same level of authority may find it hard to collaborate if each has many local problems on his hands.

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engaged on its own local problems will tend to be opaque;* the manager in charge of it will not know what is happening at its operational points, and its operators will not understand the policies of their manager.

For these reasons it is useful to introduce the second concept of experience departing from expectation. The operators will find themselves being subjected to management decisions that seem to bear no relation to the situations in which they are called upon to work. In a coalmine, where the opacity of the structure is vastly magnified by the environmental difficulties of communication of any kind, the gap between what the men expect the management to contrive in order to put things right, and what in fact the men experience from shift to shift may be very wide. It may be so wide that the men cease to hold any confidence whatever in their management; it is suggested that the gap between their expectation and their experience is a direct measure of their morale. This might explain why, in some mines of very bad geological conditions, morale can be high; the men do not expect that their management can make things significantly better, because they know the roof and roadway conditions introduce a measure of uncertainty. Provided that this level of uncertainty is not exceeded—that is, provided that the organization is not haphazard beyond an admitted level—morale is good. But, if this degree of uncertainty is exceeded, morale may decline and the accident rate or absenteeism may go up. Similarly in very good conditions, where the men expect steady work and a reliable organization, morale may suffer as a result of interruption or breakdown that the men working under bad conditions would not so much as notice. This is, no doubt, a highly *a priori* and subjective argument, but it may be useful in the application of analytical methods to the study of human relations in industries with simple structure problems, as in Coal.

It is very easy, when studying such problems with the aid of geometrical concepts such as structure and analytical methods such as correlation, to over-simplify one's methods and one's arguments. If, for example, it is true that men feel some organizations to be so haphazard that they are driven to revolt against them, it is no less true that other organizations are so minutely organized that men feel it an insult to their self-respect to submit to them. Morale may be just as bad in an organization so detailed that it deprives its servants of all initiative as in one so slipshod that it hinders

* In a family of homologous coalmines it can be shown that the opacity will be proportional to log. M.

them from achieving any satisfaction in their tasks. These are very general and perhaps unscientific statements, but it would appear that basically good morale is to be found where all the workers—managers, under-officials and wage-earners alike—have a reasonable scope to deal with their own local difficulties. These difficulties, and the powers of the official or of the workmen confronted with them, must be equally matched. When a man has both interesting problems and the means of solving them he is likely to be a more contented person than when, on the one hand, he is never called upon to use his intelligence at all, or on the other hand, he is overwhelmed by problems that he has no power to meet. In other words, the gap between his expectation and his experience must offer a challenge to him that he can meet from his own resources—and these resources include any that he can draw from elsewhere in the management structure.

The translation of these very subjective ideas into a management code will mean, at least in the coal industry, a great deal more work than has already been done. It will in particular demand that the business of every official should be considered under three distinct headings. By "business" in this context is implied all the matters with which he, as an official of the mine, will come into contact during his task. He should know these matters, inside a code recognized by the whole management team of the colliery, as: (*a*) those which he should have power and authority to settle on the spot; (*b*) those which he should deal with after consulting other officials of the colliery; and (*c*) those which are outside his field altogether and that he should therefore never touch.

There will be a different classification of these three classes at every level of authority, and the management code for the whole colliery should say what they are. The general structure problem would appear to be, firstly, the definition of these three classes inside the existing structures, and, secondly, the modifications of the structures to maximize (*a*) at all levels. It is not, of course, pretended that the study of management structure lends itself to the application of theorems dealing with maxima and minima in the sense that such theorems have application in physics or geometry; it is only too clear that, in addition to structure, the personalities of the managers and of their subordinates have the most profound influence on the morale of the miners who work for them. And the very concepts of structure are not complete merely after discussions about size of groups and so forth; the various services provided in the mine, such as the haulage system, endows it with an organic unity additional to the structure so far here discussed. Nevertheless,

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the concepts of opacity and of the threefold classification of responsibility touched on in this section apply also to these services. The most that can be said at the moment, perhaps, is this: it is highly likely that, if we so alter the structure of management of a colliery as to increase the precision with which any official in it knows what needs to be done and how to get it done, the morale of the men working in it will be improved. Perhaps this is an obvious truth; if so, this is probably why it is so easily overlooked.

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MONTE CARLO METHODS IN HEAVY INDUSTRY

by

H. G. JONES* AND A. M. LEE†

While the expanding application of operational research demonstrates that executive boards are aware of its effectiveness as an aid to scientific management, the writers wish to direct attention to the merits of one aspect of operational research whereby accurate forecasts can be made of future conditions in batch processes in which the duration of certain operational stages may vary with deleterious effects on the overall efficiency. The same method may be used to predict the benefit arising from a suggested remedy.

Before authorizing capital expenditure on extension and modification of equipment, or before making drastic changes in the existing practice of an established shop, the discriminating manager needs to estimate as exactly as possible the full implications of the proposed changes. This may present little difficulty when the process is under the precise control of the management and no operational problems arise which cannot be solved by successive application of the known logistics of the process.

With certain processes, however, the mantle of prophet is less comfortable. If, for instance, the raw materials are subject to fluctuations outside the control of the management, a variable is immediately introduced which makes accurate forecasting more difficult. If, in addition, the process requires physical conditions such as a high temperature close to the working limit of the containing vessel, uncontrolled erosion and corrosion can lead to undesirable reactions detrimental to the process, and forecasting becomes even more involved. If, to these, the limitations of the mechanical auxiliary appliances and of any subsidiary processes are added, the problem should be worthy of a scientifically obtained solution. Undoubtedly, inspired guesswork has on occasions been brilliantly successful; its many failures have ranged from the forgotten to the disastrous.

What can be done to give a scientific forecast? A statistical picture or model can be built to describe existing conditions and, from this, the general principles governing the time characteristics of the processes may be extracted. The effects of proposed changes

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can then be synthesised, and the future conditions predicted with some certainty.

Although the principles outlined in this paper are of fairly general applicability, they will be illustrated by reference to open hearth steelmaking furnaces where the capital expenditure per unit of output is particularly heavy, and hence "forecasting" is of exceptional interest.

The Problem

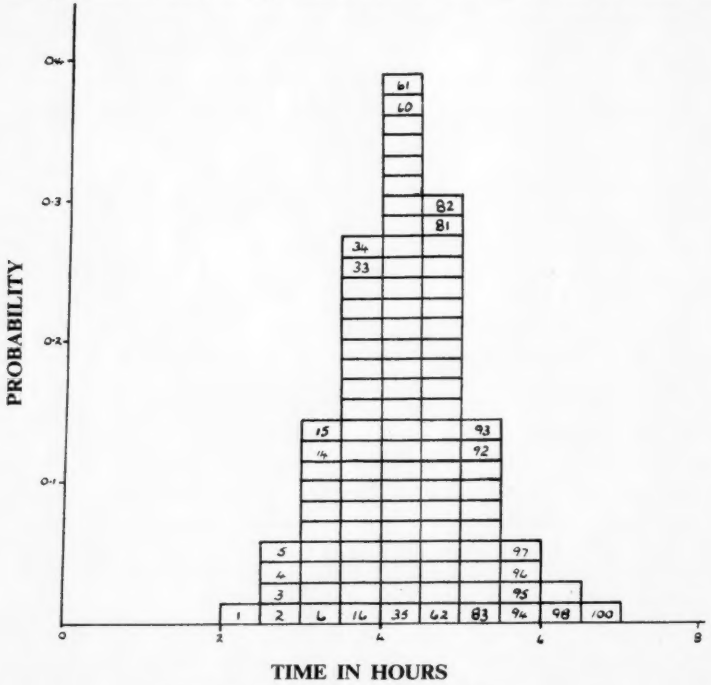
Stated very briefly, steel is made in open hearth furnaces in batches by melting steel scrap to which is added pig iron in either a solid or molten state; after refining and receiving "additions" to adjust composition, the steel is tapped into ladles and passed forwards to the next stage in the process. In a particular furnace, small by modern standards, some 80 tons of steel scrap would be charged into the furnace per batch. Since the scrap is variable in size, ranging literally from old bedsteads to heavy gearboxes, the time to charge the scrap into the furnace can quite easily vary from $2\frac{1}{2}$ hours to 6 hours, the mean time being about $4\frac{1}{4}$ hours. This variation in charging time affects total batch time and it has been established that a charging period, deviating from the mean by any amount, causes a change of roughly half that amount in the furnace time per batch. Further variations in overall batch time arise from chemical inhomogeneity in the scrap, in the iron, and in the slag-making additions to the furnace, as well as from the unknown factor of furnace performance. In total then, the batch time can easily vary from 9 hours to 15 hours with a mean of about 12 hours. A typical distribution for charging times is shown in Figure 1; the total batch time has a somewhat similar distribution about a mean of 12 hours.

In practice, to minimise capital expenditure on handling devices, several furnaces are grouped into a shop and share charging machines, ladles, cranes and so on. (In spite of this economy grouping, the initial cost of the ancillaries in a modern shop can approach, if not actually exceed, the initial cost of the production units.)

As can be expected, there will be times when several of the furnaces require simultaneously, for instance, the services of the charging machines. At such times, i.e. when these furnaces are said to have "bunched", the charging machines available may be unable to meet the increased demands, and high charging times, and hence high batch times, will result. These occasions will be followed by periods in which several of the furnaces will be simultaneously

refining, thus giving the chargers virtually no work to perform. At still other times, the furnace operations will be "separated" so that the work of chargers, cranes etc. will be spread more uniformly around the clock.

Now by using Monte Carlo methods—that is by exploiting the laws of chance—it is comparatively easy, though possibly laborious, to reconstruct empirically the output of the shop over a period of



TIME IN HOURS

FIGURE 1. *Distribution of charging times.*

a week; or better still for a period of six months, so that the influence of, and means for reducing, the various delays to the furnaces can be studied.

The Technique

An elementary but perfectly sound way of evaluating the furnace performance over a period is to divide the area of Figure 1 into 100 parts. The numbers 1 to 100 are put into a hat and a number withdrawn by chance. Suppose this number is 84. Reference to

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Figure 1 shows that 84 corresponds to a charging time of $5\frac{1}{4}$ hours, that is, the mean charging time of $4\frac{1}{4}$ hours + 1 hour. Since, as stated above, total batch time is increased (or decreased) by half the deviation from the mean, the furnace batch time would be predicted at this stage as:

(Batch Mean Time) + $\frac{1}{2}$ (Excess Charging Time) = 12 + $\frac{1}{2}$(1)
= $12\frac{1}{2}$ hours.

To embrace the chemical variability, a second number is withdrawn by chance from the hat (the number 84 meantime having been

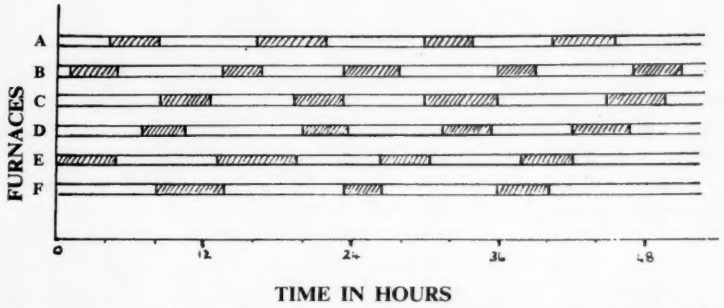

FIGURE 2. *Furnace time cycle in a hypothetical six-furnace melting shop. Shaded portions represent charging periods. Plain portions represent melting, refining, tapping, fettling periods. i.e. Total non-charging periods.*

replaced). If the new number is 27 for instance, a similar process applied to the distribution of chemical variability gives a correction factor of $-1\frac{3}{4}$ hours, leaving a final batch time of $12\frac{1}{2}$–$1\frac{3}{4}$ hours, or $10\frac{3}{4}$ hours.

Repetition enables the shop picture to be built up batch by batch and furnace by furnace. Figure 2 illustrates this pictorially for 48 hours in a shop operating six furnaces, designated by the letters A to F. Figure 2 has been carefully selected from a ten weeks' shop predicted performance to illustrate almost ideal conditions in which the furnace cycles are spread fairly uniformly over the 24 hours. It will be observed that the technique is equally applicable to skew distributions in which an excessive number of either long or short times occur.

More scientifically, a table of random numbers might have been used instead of drawing numbers from a hat. Equally well, instead of looking up the numbers of Figure 1, the correction factors could have been evaluated by multiplying the standard deviation of the distribution by the random number from tables

of standard distributions (in the appropriate units), provided the distribution is as symmetrical as Figure 1.

Charging is the period most susceptible to congestion. It is traditional in the industry that a six furnace shop should be served by three chargers. Thus if more than three furnaces are ready for charging simultaneously, some delay will occur. Numerous measurements have shown that if the charging periods of four furnaces in such a shop overlap by more than 1½ hours, then the charging of all four furnaces would be longer by about ½ hour than the times shown in Figure 1. (The times are not increased in the ratio of 4:3 for the time of interference as might at first be expected because, even if a charger was 100 per cent available for one furnace, the charger could not feed the furnace for more than 57 per cent of the time for a variety of operating reasons.) If six furnaces were ready for charging simultaneously for more than 1½ hours—fortunately a rare event—all furnaces would be delayed in charging by not more than 45 minutes; hence it is assumed for simplicity in the following arguments that charging delays are never more than 30 minutes per furnace. Thus in compiling a production time chart such as Figure 2, allowance is made of ½ hour increased charging time (and hence ¼ hour increased batch time) on each furnace if more furnaces are ready for charging than there are chargers.

Experience has shown that these hypothetical furnace time cycles, calculated by "Monte Carlo" methods as above, agree closely with the actual shop performance.

Application

Additional Equipment. An obvious line of attack with the Monte Carlo technique just described is the evaluation of the shop performance with a varying number of chargers. It is clear that an additional charger would eliminate some of the delays when the furnaces bunch, and thereby improve the performance of a six furnace shop; with the charge and batch time-characteristics quoted earlier, this increase in production is estimated to be about 2½ per cent per annum (provided the raw materials can flow at sufficient speed to serve the peak requirements of four chargers). It is not proposed to discuss the economics of this situation other than to state that in a fully integrated plant, not only must the total cost of the additional charger and its manning be estimated, but consideration must also be given to the manufacturing capacity of the subsequent units in the production line.

Similarly, the cranes handling the finished batches may be a source of minor delays at most times and of bigger delays when the

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furnaces bunch together. It is a useful exercise to evaluate, again by Monte Carlo methods, the output of a six furnace shop with two, three and four ladle cranes. It has been shown that by installing an extra ladle crane in a two crane shop, without departing from conventional handling practice, the throughput of the shop would be expected to increase by about ½ per cent per annum. This useful increase requires appreciable capital and running costs because the cranes in question are wide span and must be rated to lift at least 150 tons even in a shop with relatively small furnaces. The same increase of ½ per cent might possibly be attained by departing, again with capital costs, from orthodox practice, but it is not the purpose of the present article to enter into discussion on this point.

Development of an "Unbunching" Drill. It will have been observed that two possible changes involving capital and running costs have been considered as palliatives for loss of production arising from bunched furnaces. Might it therefore not be worthwhile studying the bunching process itself? Might it not be possible to re-organise the shop in some way with no or little capital expenditure to reduce bunching? And if bunching were reduced, what increase in throughput could be expected?

Bunching of furnaces has long been known to steelmen as one of the biggest obstacles to the smooth running of the shop, but comparatively little systematic work has been undertaken as far as the writers are aware. It is generally conceded that, at some inconvenience to the normal routine operation of the conventional shop, bunched furnaces can be separated by applying a drill designed to speed up the earlier furnaces at the expense of minor delays to the later furnaces. It cannot be denied however, that many managements are reluctant to apply such a drill because they believe that the drill would have to be applied so frequently as to interfere with the smooth working of the shop and thence to cause loss of valuable production. Moreover, it must also be admitted that some older shops are so designed that, in the present state of knowledge, it appears a physical impossibility to devise an effective procedure for separating bunched furnaces.

Granted however that the shop is so designed that an unbunching drill can be applied, the management will want to know the answers to four basic questions. How often on average will the drill need to be applied? If the furnaces have been separated by the drill, how long will they remain separated? If the drill is applied, will it cause an increase or a loss in production? If an increase in production, will the increase be big enough to justify the effort required?

In the early stages of a Monte Carlo reconstruction, the furnace operations may be spaced fairly uniformly as shown in Figure 2, but as the process cycles continue, the furnace operations will gradually come together. Both actual production experience and the Monte Carlo reconstruction show that when the furnace operations are bunched, they tend to remain bunched for several successive days. Then the operations gradually separate and in general tend to remain separated for a few days until the operations are once more bunched together.

If now an unbunching drill is postulated so that, when required, the batch time of a selected furnace can be reduced by about one hour (this may involve, besides other things, making steel to an easier but still saleable specification) and the batch time of another furnace can be increased by one hour (involving possibly slightly inferior raw scrap—which must in any case be used eventually), then the effects on output etc. can be studied by a Monte Carlo reconstruction of the shop performance under these new conditions.

It has been estimated that in a shop operating five or six furnaces with the time characteristics quoted earlier, the unbunching drill would need on average to be applied at intervals of two and a half days. Under these conditions, it is estimated that the drill would yield, over a long period, an increase in output of 2·3 per cent. Whilst this figure is not of general validity (in a melting shop where bunching causes a greater dislocation of normal operations, it could be higher), the quality of the result namely, that an increase in output would result, is probably true in most cases.

Effect of Unbunching Drill on Probability of Bunching

A few special cases have been studied in greater detail to estimate the probabilities of bunching, and of the effectiveness of the drill in opposing a return to bunched conditions. Discussion will be restricted to five furnaces operating (the sixth being off the line at frequent intervals for repair) on the grounds that when all six furnaces are operating, an effective drill would be even more beneficial than when five furnaces are in use.

If, for instance, the five furnaces finished charging at half hourly intervals, the probability of bunching during the next charging period is 95 per cent; that is, in 95 times of the next 100 similar occasions a bunch was likely to occur. This probability is made up as follows:

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		Per cent
Probability of all 5 furnaces bunching	=	49
Probability of two groups of 4 furnaces bunching	=	38
Probability of one group of 4 furnaces bunching	=	8
Total		95

If, on the other hand, the five furnaces finished charging at successive intervals of 1 hour, 1 hour, ½ hour and ½ hour, then the probability of bunching during the next charging periods would be only 75 per cent, made up as shown below:

		Per cent
Probability of all 5 furnaces bunching	=	11
Probability of two groups of 4 furnaces bunching	=	33
Probability of one group of 4 furnaces bunching	=	31
Total		75

The decrease in probability is due to some of the furnace operations being separated by greater intervals in the second case.

What in fact happens after a close bunch such as the first example is, that in successive cycles the furnace bunches gradually became less severe, thus decreasing the probability of bunches in the next cycle. The Monte Carlo tests show that, on average, bunching would in this case occur between four and five times before the furnace operations were adequately spaced if no unbunching drill was applied.

If five furnaces are bunched together, under various conditions, but with the charge and batch time-characteristics quoted earlier, it has been found that the probability of uncontrolled bunching recurring in the next cycle lies almost always between 65 per cent and 95 per cent.

If steps are taken to unbunch a furnace so that the times of the end of charging are separated at hourly intervals, the probability of bunching is reduced to 30 per cent. The cases cited above are shown in the table together with one or two other cases:

Separation of Furnaces (Hours interval between consecutive ends of charging periods)	Probability in Next Cycle
	per cent
$(\frac{1}{2}, \frac{1}{2}, \frac{1}{2}, \frac{1}{2})$	95
$(1, 1, \frac{1}{2}, \frac{1}{2})$	75
$(\frac{1}{2}, \frac{1}{2}, 1\frac{1}{2}, \frac{1}{2})$	72
$(1, 1, 1, 1)$	30
$(\frac{1}{2}, \frac{1}{2}, 2, 1)$	23
$(1, 1, 1\frac{1}{2}, 1)$	18

The aim in an unbunching drill is to increase the separation so that the risk of a run of bunching is replaced by the hope of a run of freedom from bunching.

Conclusion

The value of the Monte Carlo method in predicting peak demands on the shop services under future conditions has been demonstrated. Quantitative estimates can also be given of the influence of variations in the number of ancillary services in the shop. If desired, the effect of untried changes in shop practice can be estimated quantitatively. The technique is particularly useful in long term forward planning.

The authors wish to thank the Directors of the Steel Company of Wales Limited for permission to publish this paper.

COMMISSION GÉNÉRALE D'ORGANIZATION

June Conference on O.R.

C.G.O.S. IS A NON-GOVERNMENTAL BODY organized by industry in France to promote interest in improved methods, better productivity, etc. Its activities correspond to some extent to those in Great Britain of the Federation of British Industries, the Productivity Council and the British Institute of Management, and among these activities are short courses of training in a number of special subjects and conferences for all levels of management. The conference on Operational Research to be described was the first of its kind to be held in France and was based, to a considerable extent, on the pioneer work of the O.R. group at the Institut de Statistique de l'Université de Paris under Professor Guilbaud. It took place in Paris on June 13, 14 and 15, this year. The chairman (animateur permanent) was M. Charmont, one of the C.G.O.S. staff, and the

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papers dealt with the following topics: *Practice of O.R. in an Industrial Organization*, Professor Henon, Institut de Statistique de l'Université de Paris. *Applications of O.R.*, M. Fourgeaud, Actuaire. *A Study of O.R. Applied to Administration with Examples*, M. Darnay, sous chef de division, Comptoir Nationale d'Escompte de Paris. *Evaluation of Operating Times (Foundry Practice)*, M. Moriceau, chef ingénieur, Centre d'Etudes et de Mesure de Productivité. *Comparison of Different Production Programmes in a Cotton Weaving Mill*, M. Pigot, chef ingénieur, A.C.E.C.A. *Linear Programming*, M. Schlumberger, Ingénieur, cie. I.B.M., France. *History of an Application of O.R. to a Planning Programme in an Engineering Works*, M. Saltzmann, Ingénieur civil. *A Study of Intermediate Stocks in Production Planning*, M. Kleinmann and M. Malinet, Ingénieurs, Regie National des Usines Renault. *An Application of O.R. to Fare Levels on Paris Buses and Metro*, M. Fourgeaud. *O.R. in Great Britain, with Examples of Work in Hand*, A. W. Swan, Courtaulds, Limited. *O.R. Applied to Planning in an Engineering Works*, M. Weinberg, Ateliers Construction Oerlikon. *O.R. and Pure Research*, Professor Guilbaud, Institut de Statistique de l'Université de Paris.

The majority of the papers were by and for engineers and, indeed, anyone who has contact with the use of statistics in industry and the beginnings of O.R. in France cannot help observing that these matters are very largely in the hands of engineers or, more accurately, engineer-statisticians. In Great Britain, operational research and statistical quality control have been developed mainly by mathematicians and physicists, and engineers have been notably lacking. This difference between the two countries may be due partly to the greater emphasis on mathematics as mathematics and not purely as a working tool in the training of engineers in France, but it is also partly due to the remarkable achievements in training engineers in the use of more advanced statistics by the Institut de Statistique de l'Université de Paris under Professor Darmois.

The engineer-statistician is still an engineer concerned with the practical application of his knowledge and the general tone of the papers was "how can we apply O.R. in industry?" Most papers described case histories of actual jobs, and when newer techniques were discussed it was to speculate on how they could be used, so "the down to earth" atmosphere was noticeable. There were actual problems relating to foundry practice, "buffer stocks" (rafales) in a motor-car works, the fare structure of Paris transport, and several others. Where mathematical techniques were discussed their difficulty

was not glossed over but were described in simple, or apparently simple terms. In a lecture on linear programming, for instance, the first example concerned a M. Dupond who had 300 francs to spend on breakfast costing at least 120 francs, cigarettes costing 80 francs and a pin-ball machine.

General impressions of the conference were that it was definitely useful to all who attended, partly in the actual information assimilated and partly by bringing together those who are using or proposing to use O.R. in France. The hope may be expressed that there will be a follow-on to the conference in the formation of an Operational Research Society in France.

A. W. S.

COURSES ON OPERATIONAL RESEARCH

by

R. S. GANDER

THE TWO WEEK INTRODUCTORY COURSES on Operational Research at the Institute for Engineering Production of the University of Birmingham, which have been reported in this quarterly from time to time, continue to be the only formal courses of instruction in the subject in Europe. Recent enquiries of Birmingham indicate that Professor Parrillo, Instituto Superiore Dirigenti, Rome, Professor Brambilla, Centro de la Ricerca Operativa, Universita Bocconi, Milan, and Professor Belz, Department of Statistics, University of Melbourne, are considering similar courses. This consideration may also lie behind an enquiry from the Director, Instytut Ekonomili I Organyacji Przemyslu, Warsaw, while the Produksjonsteknisk Forskningsinstitut, Oslo, has in mind supplementing its production engineering instruction by sending selected personnel to the linear programming weeks of an advanced operational research course now being planned at Birmingham. To McCloskey's excellent article on "Training for Operations Research" (*J. Operat. Res. Soc. America*, November, 1954) which gives information on United States Courses, it is interesting to add that in Washington the G.E.C. School of Nuclear Engineering ran a night school course of 18 lectures on Operations Research, last Spring. The programme, which the Instructor, L. Wheaton Smith, Jnr., has kindly sent over, is of similar design to that of the fortnight's residential course at Birmingham.

The Birmingham Courses were started in 1953 by Dr. T. U. Matthew, first occupant since its inception in 1948 of the Lucas

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Chair in the Principles of Engineering Production. This was just after Dr. Matthew had become first Director of the Institute for Engineering Production, which he had been able to found, under the University, with another generous Lucas gift. It may be said that the other, similar, courses of the Institute, in Work Study and Production Planning and Control, are useful precursors to the Operational Research Course.

As Dr. Matthew has just left the University to return to industry this is an appropriate occasion to acknowledge his pioneer contribution in this field of education. Members of the Operational Research Society, then Club, were prominent in advising on the courses at their inception and several have since greatly assisted by lecturing. Of these, Sir Charles Goodeve of B.I.S.R.A. regularly contributes on "The Nature and Design of O.R.", Mr. A. W. Swan of Courtaulds has consistently supported on "Organising an O.R. Department in an Industrial Firm", and only recently has Mr. L. H. C. Tippett of the Shirley Institute speaking on "O.R. in the Cotton Industry" been obliged to randomise the number of his lectures. Arrangements are in hand for several of the speakers to publish modified forms of their lectures in a series of articles on Operational Research in the publication "Research".

Six courses have brought the number of members attending up to 140, including 26 from overseas. At the time of writing, the next course, which starts on 10 October, is nearly fully booked, while the eighth course which starts on 28 November is half booked. These two courses will include speakers on O.R. in agriculture, airways, building, coal, defence, land passenger transport, market research, non-ferrous metals processing, road research, sea cargoes, steel processing, telecommunications, time study. Apart from linear programming, the mathematical statistical lectures which are of an introductory cum refresher type have been put at the beginning of the course that statisticians may miss them if desired. Other speakers will offer *soupçons* indicating the relevance and value of, economics of the size and location of firms, electronic computers, industrial psychology, trade unions, work measurement.

Live case studies, brought by individual members and examined by syndicates, form an important part of the courses whose general purpose is to enable industrial managers, senior production engineers, operational research and work study specialists to meet together with leading authorities in the operational research field to engage upon discussions and group studies on modern developments and trends in the application of O.R. to industrial problems.

THE RISK TAKEN IN CROSSING A ROAD

by

JOHN COHEN,* E. J. DEARNALEY* AND C. E. M. HANSEL*

1. *Introduction.* 2. *Method of observation.* 3. *Results: (i) frequency of road-crossing against approaching traffic; (ii) maximum risk-taking levels; (iii) relationship between (a) age and (b) sex and frequency of crossing at different intervals of time.* 4. *Discussion of results and conclusions.*

1. *Introduction*

In a previous paper[2] we studied subjective probabilities in risk-taking under conditions in which (a) the subject has varying amounts of information about the task, and about his performance, and (b) his estimates relate to different levels of difficulty as measured by his success in performance. In the present paper we propose to extend further the study of certain aspects of (b). We shall be concerned with everyday behaviour.

In the previous study each subject estimated, in advance of his performance, his probable success at different levels of difficulty, the difficulty or risk being measured in terms of the proportion of successes in his actual performance and not directly in terms of units on a physical scale. "Difficulty" or risk was thus defined relatively to the individual, not in an absolute sense. For example, in a given situation, such as jumping a beam, if one raises the height of the beam, the task becomes more difficult to some extent for all subjects. But for each person there will be a particular height of the beam he will succeed in jumping, say, in 70 per cent of his attempts. Instead of measuring difficulty in terms of the absolute height of the beam, we measured it in terms of level of performance, that is to say, the difficulty of a task for any subject was measured by the proportion of his successes at any given height of the beam. One and the same height might yield different levels of performance for different subjects. Such a measure is independent of individual variations in skill; the 70 per cent point will vary in height for different individuals although it assumes the same value for all the individuals as a measure of difficulty relatively to the individual. The same holds true of any other percentage point.

In the present study we did not obtain any estimates of likely success in advance and the subjects were unaware that they were under observation. The presence of risk was measured in terms of

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group attempts to perform or not perform in varying circumstances, not in terms of the proportion of individual successes.

The behaviour studied was that of people crossing a road from a centre island to the pavement, or *vice-versa*, against one stream of traffic. It would not be far-fetched to assume that each pedestrian intended to cross the road safely without being injured by a vehicle. A person's tacit beliefs about the chances of safely crossing a road, which we regard as his subjective probability of safety (or injury), may be assumed to be related to the speed and distance of approaching vehicles.

2. *Method of Observation*

A crossing suitable for the present study was chosen on a main road in a busy part of Manchester. The full width of the road was 47 feet. There was an island in the centre of the road but no zebra crossing, traffic lights or police control. Traffic density was moderately heavy. The study extended over a period of one month and 1,189 observations were made on 491 persons.

Four kinds of observation were recorded for each member of the sample: (i) age, (ii) sex, (iii) speed and distance of vehicle, (iv) crossing or not crossing.

Age and sex were both estimated from the pedestrian's appearance and attire. A measure of the speed and distance of the approaching vehicle was obtained by recording the time from the moment the person stepped to the edge of the road and looked at the oncoming traffic to the moment when the first vehicle reached the crossing. Measurements were taken to the nearest second. Preliminary observations had indicated that when vehicles were more than 10 seconds away pedestrians without exception would cross the road. Our analysis was therefore limited to vehicles within that distance. Thus observations of whether persons did or did not cross were made in relation to the speed and distance of vehicles as measured by the time taken by them to reach the site of the crossing.

Pedestrians crossing in groups were ignored. The number of bicycles on the road was very small as compared with motor vehicles and therefore the observations made of pedestrian behaviour in relation to cyclists were not used in the present study.

3. *Results*

(i) *Frequency of road-crossing against approaching traffic.* We show in Table 1 observations made when the vehicle is at different distances from the site of crossing, distance being expressed in terms of the time the vehicle takes to reach the site. Only one person out

of 364 who had the opportunity crossed when the vehicle was 2·5 seconds or less away from the site. When the vehicle was 4 or 5 seconds away about half the people who could cross did so. When the vehicle was about 9 or 10 seconds away almost everyone crossed.

TABLE I

NUMBER AND PROPORTION CROSSING IN RELATION TO THE TIME TAKEN BY THE VEHICLE TO REACH THE SITE

Time* (secs.)	Total Observations	Number Crossing	Percentage Crossing
1	159	0	0·00
2	205	1	0·49
3	184	22	11·96
4	159	60	37·74
5	110	65	59·09
6	94	76	80·85
7	63	58	92·06
8	40	37	92·50
9	35	34	97·14
⩾10	140	138	98·57

* The class interval in each case is ± 0·5 seconds.

Our task is now to determine the nature of the relationship holding between the proportion crossing and the varying degree of objective danger, as measured by the speed and distance of the vehicle.

We have assumed that a person's subjective probability of safety, that is to say, his implicit belief about safely crossing a thoroughfare, is related to the speed and distance of oncoming traffic. We may assume further that this relationship holds not with the actual time for the vehicle to approach but with the logarithm of the time. This second assumption is perhaps rendered more plausible by the fact that in a large variety of psychological experiments a good fit is obtained between the values of the independent variable and the frequency of response if the values of the independent variable are expressed in logarithms of conventional units. This is illustrated, for example, in the psychophysical study of loudness and the intensity of sound. It is also exemplified in comparative judgments of the apparent duration of long intervals of time.[3]

In view of these assumptions and the fact that the percentage of pedestrians crossing the road increases with longer time for the car to reach the site, we should expect the distributions of these percentages, when plotted against the logarithm of the time, to fit a normal ogive. Indeed, when the percentages are plotted against log time, a good fit is obtained. An ogive was fitted by converting the

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actual percentages to probit values. A straight line was fitted to these values by the method of least squares. The expected probits and hence the expected percentages were then calculated. The value of the time interval at the 50 per cent point is 0·66 log seconds ($\sigma = 0{\cdot}15$ log seconds).

The expected frequencies were obtained by multiplying the expected percentages by the total observations given in Table I. When these expected frequencies are compared with the observed (as given in Table II) we obtain $\chi^2 = 1{\cdot}81$; d.f. $= 5$; $0{\cdot}80 < p < 0{\cdot}90$.

TABLE II

OBSERVED AND EXPECTED NUMBER CROSSING IN RELATION TO THE TIME TAKEN FOR THE VEHICLE TO REACH THE SITE

Time* (secs.)	Log Time	Number Crossing (observed)	Number Crossing (expected)
1	0·00	0	0
2	0·30	1	1
3	0·48	22	20
4	0·60	60	52
5	0·70	65	65
6	0·78	76	74
7	0·85	58	56
8	0·90	37	38
9	0·95	34	34
⩾10	1·00	138	138

* See footnote Table I.

The observed and expected proportions of pedestrians crossing at each value of log time are plotted in Figure 1.

(ii) *Maximum risk-taking levels.* Let us assume that people tend to cross a road against approaching traffic at or below a certain level of subjective probability of risk, that is, each person has a maximum risk-taking level. If he believed that there was no likelihood at all of reaching the other side of the road, he would not attempt to cross. On the other hand, only very rarely would he cross a road with a sense of absolute safety. Presumably there is a value of subjective probability of risk above which people will seldom, if ever, cross. This value of subjective probability doubtless varies between individuals and for the same individual at different times. In large samples these variations may be assumed to be smoothed out, as in the major part of Table I. Table I therefore represents the cumulative distribution of levels of maximum risk-taking.

The expected percentages crossing at any time interval (col. ii, Table III) include the percentage that would have crossed at a shorter interval. For example, 59·5 per cent are expected to cross at 5 seconds. These would doubtless also cross at longer time

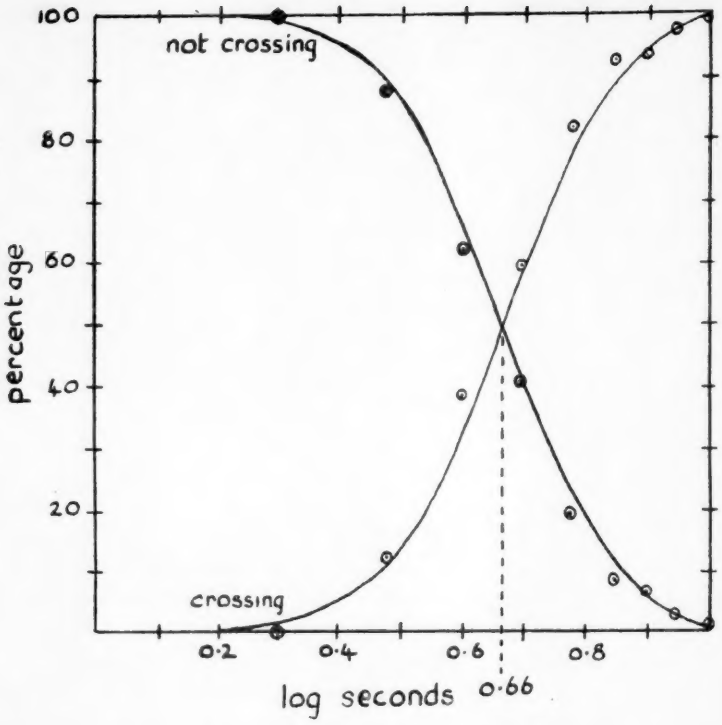

FIGURE 1. *Percentage of pedestrians crossing and not crossing at different intervals of time: best fitting ogives.*

intervals. Similarly, 33 per cent are expected to cross at 4 or more seconds. The difference, 26·5 per cent, would cross at 5 or more seconds but not at less than 5 seconds. This means that 26·5 per cent have their maximum risk-taking level at 5 seconds (or, to be more precise, between 4·5 and 5·5 seconds). These risk-taking levels are log normally distributed with a mean at 0·66 log seconds ($\sigma = 0{\cdot}15$ log seconds). The maximum risk-taking level of 50 per cent of the pedestrian population, in the circumstances under discussion, is reached when the vehicle will take 4·6 seconds to arrive at the site.

In the next section we shall see how the mean maximum risk-taking level varies with age and sex.

(iii) *Relationship between* (*a*) *age and* (*b*) *sex and frequency of crossing at different time intervals.* The 1,189 observations are

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TABLE III

DISTRIBUTION OF MAXIMUM RISK-TAKING LEVELS

Time* (secs.)	Expected Percentages Crossing	Expected Percentages Crossing at not less than the Stated Interval
1	0·00	0·0
2	0·7	0·7
3	10·8	10·1
4	33·0	22·2
5	59·5	26·5
6	78·2	18·7
7	89·4	11·2
8	94·4	5·0
9	97·3	2·9
⩾10	98·8	1·5

* See footnote, Table I.

TABLE IV

DISTRIBUTION OF AGE AND SEX IN THE SAMPLE

Age	Sex: Male	Female	Total
15 and under	54	20	74
16–30	142	262	404
31–45	247	230	477
46–60	80	111	191
61 and over	14	29	43
Total	537	652	1,189

TABLE V

MAXIMUM RISK-TAKING LEVELS FOR MALE AND FEMALE SUB-GROUPS: MEAN VALUES

	15 and under	16–30	31–45	45–60	61 and over
Males					
Mean values	0·56	0·55	0·66	0·65	0·65
σ	0·07	0·14	0·12	0·10	0·01
n	54	142	247	80	14
Females					
Mean values	0·73	0·65	0·67	0·68	0·76
σ	0·10	0·10	0·12	0·06	0·02
n	20	262	230	111	29

grouped by age and sex in Table IV. In Table V we show, for males and females separately, the log time intervals corresponding to the mean values of maximum risk-taking in each sub-group. These were calculated by fitting ogives using the same method as previously.

A good fit to a normal ogive was found in all cases where the size of the sub-group made it possible to apply a chi square test.

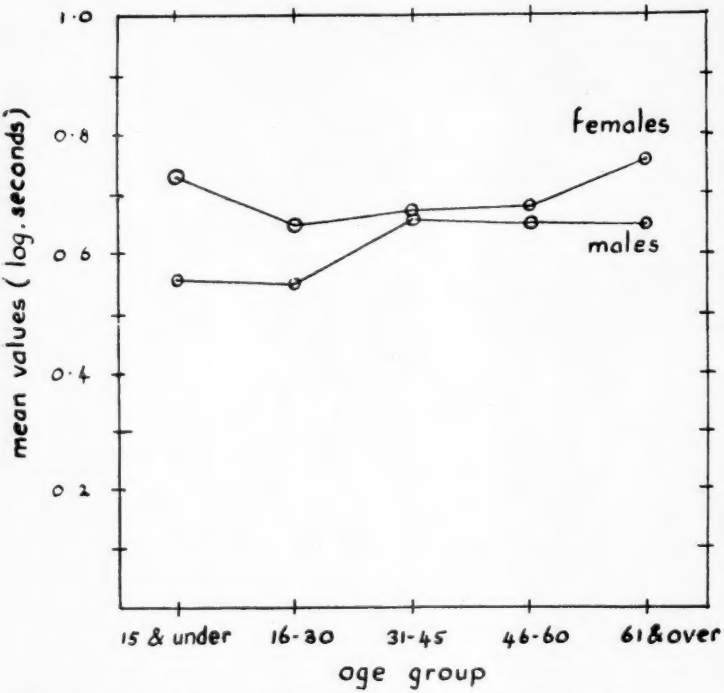

FIGURE 2. *Maximum risk taking values for male and female sub-groups: mean values.*

When comparing adjacent age groups, in the case of the males, the mean is significantly higher for the 31–45 group than for the 16–30 group. In the case of the females, the mean is lowest for the 16–30 group and increases afterwards in the higher age groups; the group aged 15 or less, however, also has a high value. The differences between the means for the two sexes in the same age groups are all significant with the exception of the 31–45 age groups. (See Figure 2.)

We can express these results in terms of clock time. The means range from 3·5 seconds in the 16–30 male age-group to 5·8 seconds in the 61+ female age-group.

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4. *Discussion of Results and Conclusions*

The problem investigated here in relation to pedestrian behaviour is general in its relevance to everyday life.[1,4] Any individual encountering daily hazards probably tends to acquire a given maximum risk-taking level in similar types of situations. The maximum risk-taking level is the point at which he will begin to undertake a given task. He will not as a rule act at a level of hazard greater than his maximum. As the level of hazard decreases he will tend to act more and more readily. We might expect that the maximum level of subjective risk-taking is much closer to an objectively reasonable "risk" in situations where there is no danger than where there is danger. We have no evidence that there is a general factor of risk-taking, either between persons or within persons.

A number of practical suggestions emerge from this study. Children's traffic sense might be improved if they received training in judging the speed and distance of approaching vehicles. Potential car drivers might also profit from such training, and appropriate tests might be introduced. The risk-taking levels at zebra-crossings might be studied, thus measuring the effectiveness of such devices. The training of men in the Services, especially in the R.A.F., opens up a field for the study of levels of maximal (and optimal) risk-taking in relation to the task and the operators.

Our conclusions may be briefly stated as follows:

1. A study was made of 491 persons of varying age in relation to crossing or not crossing a road against oncoming traffic. 1,189 observations were recorded.

2. Fifty per cent of the sample who could cross when the vehicle was about 4·5 seconds away did, in fact, cross.

3. No one crossed when the vehicle was less than 1·5 seconds away, and everyone crossed when the vehicle was more than 10·5 seconds distant.

4. When the percentages crossing at different intervals of time were plotted against the logarithms of the time intervals, a good fit to a normal ogive was obtained.

5. The maximum risk-taking levels when measured in terms of log seconds are normally distributed. These vary from individual to individual and probably from situation to situation.

6. Our study indicates the possibility of determining where controlled crossings are necessary taking into consideration the density of traffic and the time available for pedestrians to cross.

NOTE

At the time this paper was written, we were unfortunately unaware of the work carried out by R. L. Moore previously described in this journal (*Operational Research Quarterly* **4** (1)) or of relevant studies (unpublished) that have been made at the Road Research Laboratory. It seems that our results are in accord with those of previous investigators. For example, Mr. Moore observes that "pedestrians are concerned primarily with a time-gap and not a distance-gap in the traffic." He also remarks that each pedestrian presumably has his own critical distance gap which seems to refer to what we have described as "maximum risk-taking." Our finding that no one crossed when the vehicle was less that 1·5 seconds away seems also supported by earlier enquirers.

J. C., E. J. D., C. E. M. H.

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O.R. IN RUBBER

IN reviewing the work of the Research Association of British Rubber Manufacturers, the annual report of the Department of Scientific and Industrial Research for 1953–4 includes the following comment: "During the past year a start has been made with operational research, or more specifically, productivity surveys of the basic operations of masticating and mixing unvulcanized rubber. These operations were selected because they are common to most rubber factories and represent an annual expenditure of some £3 million.

"A grant of nearly £3,000 has been made from Conditional Aid funds, and this has enabled a start to be made on this research project. There has been a good response from member firms to requests for permission to survey their mill room operations; indeed, some now ask the research team to visit them. It is too early to state any detailed conclusions, since the comparative productivities between one factory with another have not yet been worked out. However, the investigations have already revealed several causes of low productivity, such as excessive time required for cutting up bales of plantation rubber (often aggravated by their having become mis-shapen during transit), excessive idling time of machinery, or unnecessarily long mixing times.

"There can be no doubt that the results of these surveys, if taken to heart by the factory managements—and some are already taking action—will lead to an increase in productive efficiency."

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EDITORIAL NOTES

A Library Service

ONE OF THE ORIGINAL justifications for burdening the world with another journal when this Quarterly was founded six years ago, was the need to draw together the literature relevant to this subject. Its publication is scattered over the whole field of technical literature, and this has led the joint editors to regard the provision of abstracts as one of the primary functions of the Quarterly.

To achieve this we have hitherto relied largely on our own efforts supplemented by assistance, which we gratefully acknowledge, from correspondents on specialized areas of science and technology. The result has been that, while we hope that most of the important references have been caught in our net, our abstracting has not been by any means as methodical as it should have been. Meanwhile, literature on the subject has increased, and we are now bound to regard the situation as unsatisfactory, and to consider how the service may be put on a better, perhaps a more professional, basis.

It would appear that the ideal would be to have a specialized Operational Research Library, with a full-time librarian and abstractor. Unfortunately, this would require an outlay far beyond the resources of this Quarterly. Such a library, however, might serve a far wider purpose than the provision of abstracts, and it may well be that some readers would be willing to pursue the matter further.

We therefore seek your advice as to whether such a library service is needed and would be supported. Preliminary discussions have already elicited an encouraging offer of accommodation and administrative

services within a suitable existing organization, which might cut down the cash requirements to a manageable level. We should now very much value information, comment and suggestions from our readers in general. Specifically, we should like to know whether they or their firms would be willing to subscribe to such a scheme, what sort of subscription they would think worthwhile, and what sort of service they think would be most useful. It might be useful to provide a point of departure for discussion by mentioning that if we could enrol a dozen firms each to subscribe 50 to 100 guineas a year the scheme would probably become practicable.

Operational Research Society

THE ANNUAL GENERAL MEETING of the Operational Research Society was held at the Council Room of the Agricultural Research Council, London, on October 13. The annual report of the committee for 1954–55 stated that the membership on 31 July, 1954, stood at 101. At the annual general meeting held on 13 October, 1954, it had been agreed that a grade of Associate should be formed. This grade would be open to people having an interest in operational research, but who would not claim to be professional workers in the field. During the past year 22 associates and 51 members had been elected. There had been 1 resignation and the membership on 31 July, 1955, stood at 173, comprising 152 full members and 21 associates.

During the twelve months to 30 June, 1955, the Operational Research Quarterly had published four issues containing 166 pages. This was easily the highest number of pages yet, and compared with 100 pages in the previous year. Increased operating costs had been foreseen by the editors and accepted as a matter of policy in the belief that an increased subscription list would result. There were signs that the assessment was correct, and the paid circulation figures at 30 June, 1955, were (1954 figures in brackets): Members 155 (88); Great Britain 274 (274); U.S.A. 255 (218); Other countries 170 (145); Totals 854 (725).

Once again the Society owed a very great debt of gratitude to Dr. Peter B. Myers, the honorary subscription agent in the United States. Without his unrelaxing enthusiasm and efficiency it was to be doubted whether the Society could continue to publish its Quarterly independently of a commercial publisher or professional subscription agency, with the diminished control that that implied.

On the retirement of Sir Owen Wansbrough-Jones as President of the Society after two years in office, Sir William Slater was elected President. In the place of D. Hicks, who retired from the Committee in rotation, H. G. Jones was elected to the Committee.

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GENERALIZATIONS OF THE WAREHOUSING MODEL

by

A. CHARNES * AND W. W. COOPER †

1. *Introduction*

ONE TASK OF MANAGEMENT SCIENCE which is beginning to emerge from recent work in mathematical programming involves the study of management problems in order to develop special classes of models and instruments of analysis. Typical specimens are supplied by transportation models, problems of personnel assignment, and plant (and machine) loading. Balance problems of production scheduling, optimum blends of chemical components, and input-output analyses directed to military mobilization studies, are other examples.

Broad features of such models have been identified and are being used for purposes of further analysis and study. One objective is to relate seemingly separate types of problems and, in so doing, to evolve effective means for handling a wide variety of situations. For example, problems in personnel assignment and machine loading have been related to features which are prominent in transportation models. Methods which were first applied to blending aviation gasolines have been extended to a variety of other problems, including the manufacture of formula feeds for animals.

Only a few years ago emphasis was placed on abstract research designed to establish the fundamental unity of programming (planning) problems. Work in the area of linear programming and its associated general techniques, such as the simplex method, supplied one focus.[6, 11] Effort devoted to establishing relations with other major disciplines such as game theory and statistical decision theory provided another focus.[2, 14] This work is continuing apace, although the fundamental underpinnings of mathematical programming may now be regarded as well secured.

Increasing attention to applied work has added new directions to programming research. A natural outgrowth of this work has been the initiation of a series of researches directed to identifying and relating significant features in a wide variety of managerial problems. Analysis of the models developed for dealing with supposedly different problems has barely begun, so that a large task of model development and classification lies ahead. Closely associated with this work is the evolution of new techniques which are

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designed to take full advantage of the special features of each such class of models.

One of the classical problems of linear programming was the so-called warehousing problem. First formulated by A. Cahn,[1, 12] it may be paraphrased as follows: "Given a warehouse with fixed capacity and an initial stock of a certain product which is subject to known seasonal price and cost variations, what is the optimal pattern of purchasing (or production), storage and sales?" When formulated analytically this problem can be (and was) identified as a linear programming problem. At the time that this problem was developed it was natural to regard such identification as the terminal point of research.

One objective of the present paper is to generalize the warehousing problem in order to cover multiple products (and warehouses) and varying prices. Special methods will then be presented which take advantage of the structure of such problems in order to evolve efficient computational devices. These methods rest on the dual theorem and the "regrouping principle". This theorem and this principle, which were evolved in other contexts,[7, 14] will be restated here. Numerical examples will also be supplied, so the reader can see how the methods are applied in the context of a concrete illustration. The examples are sufficiently simple, so the reader can verify the results secured or proceed to solve the problems by other means in order to be sure that he has understood what is involved.

The warehousing problem is of interest in its own right. It is also worthy of study for other reasons. On the one hand it represents a generalization of simpler transportation models; on the other hand it is the simplest example of a large and important class of dynamic models. Insight into both areas is therefore provided by a study of general warehousing models.

2. *Analytical Formulation of the Model—Single Product Case*

The following analytical representation of the warehousing problem will provide a basis for subsequent discussion. Let

B = the fixed warehouse capacity,

A = the initial stock in the warehouse.

Consider a seasonal product to be bought (or produced) and sold for each of $i = 1, 2, \ldots, n$ periods. For the i^{th} period, let

c_i = cost per unit,

p_i = selling price per unit,

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x_i = amount bought (or produced),

y_i = amount sold.

The constraints are as follows:

(1) Buying constraints: The stock on hand at the end of the i^{th} period cannot exceed the warehouse capacity.

(2) Selling constraints: The amount sold in the i^{th} period cannot exceed the amount available at the end of period $(i-1)$.

(3) Non-negativity constraints: Amounts purchased or sold in any period are non-negative.

In mathematical form, the buying constraints become

$$A + \sum_{j=1}^{i} (x_j - y_j) \leqslant B, \qquad i = 1, 2, \ldots, n. \tag{1}$$

The selling constraints become

$$y_i \leqslant A + \sum_{j=1}^{(i-1)} (x_j - y_j), \qquad i = 1, 2, \ldots, n. \tag{2}$$

Note: For $i = 1$, this condition reduces to

$$y_1 \leqslant A,$$

For $i = 2$, the selling constraint is

$$y_1 + y_2 \leqslant A + x_1,$$

and so on.

The non-negativity requirements are

$$x_j, y_j \geqslant 0, \quad \text{for} \quad j = 1, 2, \ldots, n. \tag{3}$$

The objective is to maximize the profit functional

$$\pi = \sum_{j=1}^{n} (p_j y_j - c_j x_j), \tag{4}$$

subject to the buying, selling, and non-negativity constraints.

The constraints may now be rewritten for convenience in obtaining the dual formulation:

$$\sum_{j=1}^{i} x_j = \sum_{j=1}^{i} y_j \leqslant B - A, \qquad i = 1, 2, \ldots, n. \tag{1.1}$$

$$-\sum_{j=1}^{(i-1)} x_j + \sum_{j=1}^{i} y_j \leqslant A. \tag{2.1}$$

$$x_j, y_j \geqslant 0, \tag{3.1}$$

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and the functional (to be maximized) may be written as:

$$\pi = \sum_{j=1}^{n} (-c_j x_j) + \sum_{j=1}^{n} p_j y_j. \tag{4.1}$$

3. *The Dual Formulation*

The above presentation may be re-phrased in vector notation and related to the usual statement of the *direct* problem of linear programming in the following form:

(i) *Direct problem*

Maximize

$$\pi = (p_1, \ldots, p_k, \ldots, p_n; -c_1, \ldots, -c_k, \ldots, -c_n) \begin{pmatrix} y_1 \\ \cdot \\ \cdot \\ \cdot \\ y_n \\ x_1 \\ \cdot \\ \cdot \\ \cdot \\ x_n \end{pmatrix} = p'\lambda,$$

where λ is subject to the following linear constraints:

$$M\lambda \leqslant b \quad \text{and} \quad \lambda \geqslant 0,$$

in which M, b and 0 are understood, as usual, to be rectangular matrices of appropriate order.

Utilizing data provided by the constraints which appear in the direct problem, a related, dual, minimization problem may also be stated as follows:

(ii) *Dual problem*

Minimize $\qquad E = w'b$

subject to the linear constraints:

$$M'w \geqslant p, \; w \geqslant 0$$

in which M' and p are the transposed matrices of the original problem and

$$w' = (t_1, \ldots, t_k, \ldots, t_n; u_1, \ldots, u_k, u_{k+1}, \ldots, u_n).$$

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The variables in these two problems are connected by a dual theorem which may be stated as follows:

*Duality Theorem.** If a solution to either (i) or (ii) exists and is finite, then a solution to the other exists, which is also finite and the optimal values are equal, i.e.,

$$\max \pi = \min E.$$

If the constraints of the warehousing problem are set forth in detail, a special "triangular" structure emerges. This structure invites the development of special methods of computation and analysis. To exhibit this fact, the constraints of the direct warehousing problem may be written in full detail as follows:

$$\begin{array}{llll}
x_1 & -y_1 & \leqslant B-A & \\
x_1+x_2 & -y_1-y_2 & \leqslant B-A & \\
\cdot \quad \cdot & \cdot \quad \cdot & \cdot & \\
x_1+x_2 & +x_n-y_1-y_2-\ldots & -y_n \leqslant B-A & (5) \\
0 & +y_1 & \leqslant A & \\
-x_1 & +y_1+y_2 & \leqslant A & \\
-x_1-x_2 & +y_1+y_2+y_3 & \leqslant A & \\
\cdot \quad \cdot & \cdot \quad \cdot & \cdot & \\
-x_1-x_2-\ldots-x_{n-1}-0 & +y_1+y_2+y_3+\ldots+y_n & \leqslant A &
\end{array}$$

If the coefficients of these inequalities (which are all $+1$, -1, or 0) are detached and set forth in matrix array they can be schematically portrayed by the triangles shown in (6). The first constraint in (5) is obtained by selecting a $+1$ from the corner of the triangle in the upper left-hand section and a -1 from the corner of the triangle in the upper right-hand section to yield $+x_1-y_1 \leqslant B-A$. Continuing in this fashion the inequality formed from the base of the first pair of triangles may be written

$$x_1+\ldots+x_k+\ldots+x_n-y_1-\ldots-y_r-\ldots-y_n \leqslant B-A,$$

as in (5).

Turning attention now to the bottom two triangles it may be noted that the corner of the left-hand figure begins one position lower than the right-hand figure. The relevant inequalities are $0+y_1 \leqslant A$; $-x_1+y_1+y_2 \leqslant A$; and, for the $(k+1)^{st}$ row,

$$-x_1-x_2-\ldots-x_k+y_1+y_2+\ldots+y_k+y_{k+1} \leqslant A;$$

* This theorem was conjectured by von Neumann and proved by Gale, Kuhn and Tucker (see Chapter XIX in reference 14) who also thereby established fundamental relations between linear programming and the theory of games.

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and so on, until finally, the inequality

$$-x_1-x_2-\ldots-x_{n-1}+y_1+\ldots+y_2+\ldots+y_n\leqslant$$

is obtained.

Treating the unit costs and prices as an extension of the table for the constraints and dropping the x's and y's at the top of the table into their proper position, the functional to be maximized in the direct problem is immediately written as

$$-c_1x_1-\ldots-c_kx_k-\ldots-c_nx_n+y_1p_1+\ldots+y_rp_r+\ldots+y_np_n$$

by reference to the bottom row.

Noting that the dual is obtained from the direct problem by transposing the structural matrix, reversing the inequalities, and inserting p in place of b, this schematic array may also be used to obtain the dual restrictions by moving the t's and u's into proper position and forming inequalities down the columns instead of across the rows. Thus

$$t_1+\ldots+t_k+\ldots+t_n-0u_1-u_2-\ldots-u_k-u_{k+1}-\ldots-u_n\geqslant-c_1$$

represents the first constraint, and

$$-t_n+u_n\geqslant p_n$$

represents the last constraint of the dual problem. Continuing into the last column in the same manner, the dual functional (to be minimized) can be written as

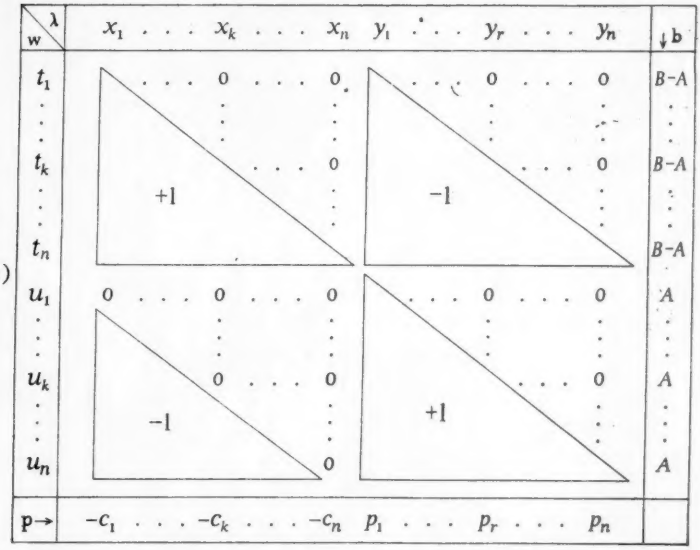
$$(B-A)t_1+\ldots+(B-A)t_k+\ldots \\ +(B-A)t_n+Au_1+\ldots+Au_k+\ldots+Au_n.$$

The tabular array set forth in diagram (6) and further discussed in reference 7 therefore provides a convenient device both for mnemonic purposes in setting forth the direct and dual problems and for studying the structure of these problems for computation and analysis.

The $\lambda=(x,y)$ and $w=(t,u)$ are, of course, the direct and dual variables, respectively. The interpretation of the former variables is clear. The latter provide a convenient means, as will subsequently be shown (and is discussed in reference 6), for evaluating the restrictions imposed by the initial inventory A, and the warehouse capacity B. In addition, a schematic portrayal, such as that set forth in (6), helps the user to determine whether it is more advantageous to obtain a solution to any problem by means of the direct formulation or by means of its dual. (The simplex method automatically provides solutions to both problems; hence the user has

a choice of routes by which to proceed. If other methods of computation are employed, the user can obtain the value of the functional for the alternate problem but will, in general, not be able to obtain the values of the variables which yield this optimum unless he also solves the alternate problem by inverting a matrix. It is the purpose of this paper to show that this is not necessary in the warehousing problem.)

(6)



Proceeding, then, to form the dual problem more compactly, reference may be had to the k^{th} column of (6) which reveals the constraints to be of the following form:

$$\sum_{i=k}^{n} t_i - \sum_{i=k+1}^{n} u_i \geqslant -c_k, \qquad k = 1, 2, \ldots, (n-1) \tag{7}$$

$$t_n \geqslant -c_n.$$

Examination of the r^{th} column in similar fashion yields

$$-\sum_{i=r}^{n} t_i + \sum_{i=r}^{n} u_i \geqslant p_r, \qquad r = 1, 2, \ldots, n, \tag{8}$$

with, of course,

$$t_i, u_i \geqslant 0. \tag{9}$$

The dual functional, to be minimized, is, by reference to the last column,

$$(B-A)\sum_{i=1}^{n} t_i + A\sum_{i=1}^{n} u_i. \tag{10}$$

4. *Transformation of Variables*

Observing the form in which the t's and u's enter the functional of the dual it is apparent that cumulative sums are simpler to consider. Let

$$\begin{aligned} T_k &\equiv \sum_{i=k}^{n} t_i \\ U_k &\equiv \sum_{i=k}^{n} u_i. \end{aligned} \tag{11}$$

Thus

$$\begin{aligned} &T_n = t_n, \\ &U_n = u_n, \\ &T_{r-1} \geqslant T_r, \qquad r = 1, \ldots, n, \\ &U_{k-1} \geqslant U_k, \qquad k = 1, \ldots, n \end{aligned} \tag{12}$$

where $U_0 \equiv U_1, T_0 \equiv T_1$. These extra inequalities are the price paid for the simplifications in the form of the functional and the constraints, viz.:

Minimize $\qquad E \equiv (B-A)T_1 + AU_1,$

subject to

$$\begin{aligned} &\text{(a)}\ T_k \geqslant U_{k+1} - c_k, \qquad k = 1, 2, \ldots, (n-1), \\ &\text{(b)}\ T_n \geqslant -c_n, \\ &\text{(c)}\ T_{r-1} \geqslant T_r, \qquad r = 1, 2, \ldots, n, \\ &\text{(d)}\ U_r \geqslant T_r + p_r, \qquad r = 1, 2, \ldots, n, \\ &\text{(e)}\ U_{r-1} \geqslant U_r, \qquad r = 1, 2, \ldots, n, \\ &\text{(f)}\ T_r, U_r \geqslant 0, \qquad r = 1, 2, \ldots, n, \end{aligned} \tag{13}$$

where the inequalities (12) have been incorporated in (13). Note that if $c_n \geqslant 0$ the inequality $T_n \geqslant -c_n$ is redundant.

5. *Computational Algorithm*

The algorithm for determining the solution to the dual of the warehousing problem now becomes apparent. The process is as follows:

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Determine alternately T's and U's by the condition that they be as small as possible, starting with T_n, then U_n, then T_{n-1}, followed by U_{n-1}, etc.

For example, the smallest possible value for T_n is the larger of $-c_n$ and 0, since from the inequalities these are the only two quantities limiting T_n from below. T_n being thus determined, the smallest value for U_n is, similarly, the larger of the two values T_n+p_n and 0. U_n and T_n being determined, T_{n+1} is the largest of the values U_n-c_{n-1}, T_n, and 0. T_{n-1} being determined, U_{n-1} is the largest of the values $T_{n-1}+p_{n-1}$, U_n, and 0. The process is continued in this same straightforward fashion to yield an optimal solution.

6. *Regrouping Principle*

The optimal programme of the dual problem and the resulting (optimal) values of the dual and direct problems having thus been established, there remains now the task of determining the values of the programme variables, $\lambda = (x, y)$, for the direct problem. This may be done by reference to the regrouping principle, which may be stated as follows for application to this problem:*

Regrouping principle. Let $\bar{w}$ be an optimal solution of $w \geqslant 0, M'w \geqslant p$, minimize $E = w'b$, such that $\bar{w}$ is expressible literally in terms of the components of p. Regroup $\bar{w}'b$ to exhibit the coefficients of p—e.g., $\bar{w}'b = p'\bar{\lambda}$. Then, if $\bar{\lambda}$ satisfies $\lambda \geqslant 0$, $M\lambda \leqslant b$, it also maximizes $\pi = p'\lambda$ subject to these conditions. By carrying along the literal t_r's and u_r's, the values T_1 and U_1 are obtained in terms of the literal c_r's and p_r's. The functional, E, may therefore be expressed in terms of the c_r's and p_r's. By the regrouping principle an optimal solution to the direct problem is obtained by setting x_j equal to the negative of the coefficient of c_j and by setting y_j equal to the coefficient of p_j.

7. *Illustration*

The above exposition may be illustrated by solving explicitly the class of cases in which

$$\begin{aligned} &c_n \geqslant 0, \\ &p_r \geqslant c_r \geqslant 0, \\ &p_{r+1} \geqslant c_r. \end{aligned} \tag{14}$$

* See reference 7 for a general formulation and development of the regrouping principle and associated theorem.

These conditions may be stated verbally as follows: (1) acquisition costs are non-negative, and (2) the selling prices expected to obtain in the current *and* next succeeding period are not below current acquisition cost.

The constraints (13) require

$$U_r \geqslant T_r + p_r \geqslant U_{r+1} - c_r + p_r$$

and, therefore,

$$U_r \geqslant U_{r+1} + (p_r - c_r).$$

But, $p_r \geqslant c_r$, so that the inequality $U_r \geqslant U_{r+1}$ becomes redundant.

Similarly, $$T_k \geqslant U_{k+1} - c_k \geqslant T_{k+1} + p_{k+1} - c_k$$

implies $T_k \geqslant T_{k+1}$ when $p_{k+1} \geqslant c_k$. Therefore, the extra constraints (12) may be disregarded. Now applying the algorithm the following results are successively obtained:

$$\begin{aligned}
T_n &= 0, \\
U_n &= T_n + p_n = p_n, \\
T_{n-1} &= U_n - c_{n-1} = p_n - c_{n-1}, \\
U_{n-1} &= T_{n-1} + p_{n-1} = p_n - c_{n-1} + p_{n-1}, \\
T_{n-2} &= T_{n-1} - c_{n-2} = p_n - c_{n-1} + p_{n-1} - c_{n-2}, \\
U_{n-2} &= T_{n-2} + p_{n-2} = p_n - c_{n-1} + p_{n-1} - c_{n-2} + p_{n-2}.
\end{aligned} \tag{15}$$

The procedure thus consists of alternately adding a "p" and subtracting a "c", the T's ending with a "c" and the U's ending with a "p" of the same subscript. Continuing in this fashion, the results

$$\begin{aligned}
T_1 &= p_n - c_{n-1} + p_{n-1} - c_{n-1} + - \ldots + p_2 - c_1, \\
U_1 &= p_n - c_{n-1} + p_{n-1} - c_{n-1} + - \ldots + p_2 - c_1 + p_1,
\end{aligned}$$

are obtained. Therefore

$$\begin{aligned}
\min E &= (B - A) T_1 + A(T_1 + p_1) \\
&= BT_1 + Ap_1 \\
&= Ap_1 + B\left(\sum_{j=2}^{n} p_j - \sum_{j=1}^{n-1} c_j \right) \\
&= Ap_1 + \sum_{j=2}^{n} Bp_j - \sum_{j=1}^{n-1} Bc_j = \max \pi,
\end{aligned} \tag{16}$$

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by the dual theorem. The optimal direct solution, obtained by applying the regrouping principle is, therefore,

$$
\begin{aligned}
y_1 &= A, \\
y_j &= B, \qquad j = 2, \ldots, n, \\
x_j &= B, \qquad j = 1, \ldots, (n-1), \\
x_n &= 0.
\end{aligned}
$$

In the first period the entire initial stock is sold. In each succeeding period purchases and sales are effected to capacity, except that no acquisitions are made in the last period.

8. ***Numerical Illustration***

Of course, the conditions of (14) are too stringent to meet with operating requirements of a more general class of cases. To fix ideas it may therefore be well to present a simple numerical example to illustrate the method more generally. It may be helpful to think of an example constructed along the following lines. A vendor purchases from a single supplier under certain terms. The supplier will allow him book credit, interest free, up to 200 tons of the merchandise at any one time, all accounts to be settled within five "periods" from the present date. As a further accommodation, the supplier will ship up to 200 tons to any customer designated by the vendor during any one period. At least one "period" must be allowed between issuance of instructions by the vendor and receipt of the merchandise by customers. The latter pay cash on delivery for all merchandise received. In American commercial terminology the relationship would be characterized as a contract for "drop shipments", with the vendor simply acting as a go-between for the supplier and the customers without handling the goods himself.

Suppose, then, that the vendor is faced with the schedule of purchase and selling prices for the periods indicated in Table I.

TABLE I

PURCHASE AND SALE PRICE QUOTATIONS FOR FIVE TIME PERIODS*

$B = 200$ tons, $A = 100$ tons

Period	Purchase price $/ton	Selling price $/ton
1	25	20
2	25	35
3	25	30
4	35	25
5	45	50

* Note these prices do not conform to the conditions prescribed in (14).

Suppose, further, that, prior to receiving any orders, the vendor has committed himself to the purchase of 100 tons of merchandise from the supplier. (Since the commitment is assumed to be a firm one, the price is irrelevant to optimization. That is, the commitment represents a "fixed cost". Under the doctrine of "opportunity cost", relevant to profit maximizing decisions of this kind, these costs do not enter into the decisions, even though the historical outlay involved must be deducted from the variable profit in order to arrive at the "true profit". See reference 7.) He wishes now to know what pattern of sales and purchase arrangements to effect in order to maximize his net return for these five periods.

Turning to the restrictions set forth in (13) and applying the computational algorithm of Section 5, the tabular array of Table II may be obtained. Each T and U is, as noted in the algorithm, set at the minimum non-negative value which the restrictions allow. (It should be remembered that this algorithm is applied to the transformed dual (minimization) problem.) Since $c_5 = 45 > 0$, the restriction (13b), $T_n \geqslant -c_n$, is redundant. The minimum value for T_5 is determined by the non-negativity requirement (13f), $T_n \geqslant 0$, at $T_5 = 0$. This value is therefore inserted at the top of the first column of Table II and the other values are derived as indicated by the formulas listed in Table II.

TABLE II

ALGORITHM FORMULAS AND CALCULATIONS

$B = 200$, $A = 100$

T		U	
$T_k \geqslant U_{k+1} - c_k$	$T_{r-1} \geqslant T_r$	$U_r \geqslant T_r + p_r$	$U_{r-1} \geqslant U_r$
$T_5 = 0$		$U_5 = T_5 + p_5 = 0 + 50 = 50$	
$T_4 = U_5 - c_4 = 50 - 35 = 15$		*	$U_4 = U_5 = 50$
$T_3 = U_4 - c_3 = 50 - 25 = 25$		$U_3 = T_3 + p_3 = 25 + 30 = 55$	
$T_2 = U_3 - c_2 = 55 - 25 = 30$		$U_2 = T_2 + p_2 = 30 + 35 = 65$	
$T_1 = U_2 - c_1 = 65 - 25 = 40$		†	$U_1 = U_2 = 65$

* The restriction (13e), $U_{r-1} \geqslant U_r$, is not fulfilled in this column, since $U_4 = T_4 + p_4 = 15 + 25 < U_5 = 50$.

† The restriction (13e) is not fulfilled since the formula

$$U_1 = T_1 + p_1 = 40 + 20 = 60,$$

which is smaller than the value secured for $U_2 = 65$.

It is perhaps worth noting that all of the T's automatically satisfy restriction (13c), which is therefore redundant in this case and may be ignored, since $T_1 \geqslant T_2 \geqslant T_3 \geqslant T_4 \geqslant T_5$ simply by virtue of the restrictions $T_k \geqslant U_{k+1} - c_k$. This is not the case, however, for

the second two columns listed under U. In the second and fourth rows the minimal values for $U_r \geqslant T_r + p_r$ are inadmissible by virtue of the restriction $U_{r-1} \geqslant U_r$. It is the latter restrictions which therefore determine the lowest values which U_4 and U_1 can assume, as noted underneath Table II.

The value of an optimal programme is determined immediately by substitution in the formula for the functional as listed at the top of (13)

$$\begin{aligned} \text{minimum } E &\equiv (B-A)T_1 + AU_1 = (200\text{–}100) \times 40 + 100 \times 65 \\ &= \$10{,}500 = \text{maximum } \pi, \end{aligned}$$

where $B = 200$ ton credit limit,

$A = 100$ ton starting commitment.

The equality (at an optimum) between minimum E (dual problem) and maximum π (direct problem) follows from the duality theorem of Section 3. There remains, however, the problem of determining a purchase-sale pattern over the five periods which will achieve this optimal return. The difficulty of ascertaining an optimum programme may be resolved by means of the regrouping principle discussed in Section 6. For, as portrayed in Table III, the transformed dual variables, T and U, can be expressed literally in terms of the p's and c's of the functional for the direct problem (cf. (4)).

TABLE III

FORMULAS FOR REGROUPING

$T_k = U_{k+1} - c_k$	$U_r = T_r + p_r$ or $U_{r-1} = U_r$ as necessary
$T_5 = 0$	$U_5 = p_5$
$T_4 = U_5 - c_4 = p_5 - c_4$	$U_4 = p_5$
$T_3 = U_4 - c_3 = p_5 - c_3$	$U_3 = T_3 + p_3 = (p_5 - c_3) + p_3$
$T_2 = U_3 - c_2 = (p_5 - c_3 + p_3) - c_2$	$U_2 = T_2 + p_2 = (p_5 - c_3 + p_3 - c_2) + p_2$
$T_1 = U_2 - c_1 = (p_5 - c_3 + p_3 - c_2 + p_2) - c_1$	$U_1 = U_2 = p_5 - c_3 + p_3 - c_2 + p_2$

These results are derived by applying the formulas of (13) to the relevant restrictions noted in Table II. Substituting in the formula for E and regrouping,

$$\begin{aligned} \min E = (B-A)T_1 + AU_1 &= (B-A)(p_5 - c_3 + p_3 - c_2 - c_1) + \\ &\quad + A(p_5 - c_3 + p_3 - c_2 + p_2) \\ &= Bp_2 + Bp_3 + Bp_5 - (B-A)c_1 - Bc_2 - Bc_3 = \max \pi. \end{aligned}$$

The resulting coefficients of the relevant p's and c's are, by the regrouping principle, identical with the coefficients—i.e., the corresponding λ's—of the functional in the direct problem. Hence, a

programme, or pattern of purchases and sales, which yields an optimum value of $10,500 is as portrayed in Table IV. This pattern yields total receipts of $23,000 and total outlays of $12,500 for a net return of $10,500. While the vendor may (and should) wish to deduct the cost of his initial "inventory commitment" in order to arrive at his actual profit, no other programme will yield a greater return to him over these five intervals of time under the stated conditions.*

TABLE IV

OPTIMAL PROGRAMME

	x = purchase				y = sales			
Period		Quantity purchased (tons)	Price $/ton	Total cost ($)		Quantity sold (tons)	Price $/ton	Total receipts ($)
0	$x_0 = A$	= 100	*	*		†	†	†
1	$x_1 = B - A$	= 100	25	2,500	$y_1 = 0$ =	0	20	0
2	$x_2 = B$	= 200	25	5,000	$y_2 = B$ =	200	35	7,000
3	$x_3 = B$	= 200	25	5,000	$y_3 = B$ =	200	30	6,000
4	$x_4 = 0$	= 0	35	0	$y_4 = 0$ =	0	25	0
5	$x_5 = 0$	= 0	45	0	$y_5 = B$ =	200	50	10,000
Total		600	†	12,500		600	†	23,000

* See page 142.
† Not relevant.

9. *Interpretation of the Dual Variables*

The calculational simplicity of this approach, using the dual formulation and the regrouping principle, should now be apparent. Extra information which can be put to use is also supplied by referring to the values secured for the dual variables along the way. The dual formulation of Section 3 shows that the w's in the dual functional, $E = w'b$, are applied to the restrictions, b, of the direct problem. The values of these variables therefore provide a means for determining increases or decreases in profits which may be secured per unit alteration in these restrictions.

Suppose that the vendor wishes to know (under the conditions of the problem stated in the preceding section) how much can be gained by increasing his "warehouse" capacity and his initial inventory. Since the regrouping principle applies in reverse (from the direct to the dual problem),[7] the relevant variables are supplied by the dual

$$w' = (t_1, \ldots, t_k, \ldots, t_n; u_1, \ldots, u_k, u_{k+1}, \ldots, u_n).$$

* There may, however, be other programmes which will yield equal returns. See reference 6 for a discussion of locating such "alternate optima".

Table III may be used to secure the necessary formulas:

$$\begin{aligned} t_5 &= 0 & u_5 &= p_5 \\ t_4 &= p_5 - c_4 & u_4 &= 0 \\ t_3 &= c_4 - c_3 & u_3 &= p_3 - c_3 \\ t_2 &= p_3 - c_2 & u_2 &= p_2 - c_2 \\ t_1 &= p_2 - c_1 & u_1 &= 0 \end{aligned}$$

These results are obtained by substituting in the definitions for T_k and U_k, as given in (11), and solving for each t_i and u_i.

Inserting these values in (10), the following results are obtained:

$$\begin{aligned} E &= (B-A)\sum_{i=1}^{n} t_i + A\sum_{i=1}^{n}(u_i) = (B-A)(p_2+p_3+p_5-c_1-c_2-c_3)+ \\ &\quad +A(p_2+p_3+p_5-c_2-c_3) \\ &= B(p_2+p_3+p_5-c_1-c_2-c_3)+Ac_1 \\ &= 40B+25A. \end{aligned}$$

Each additional ton of opening inventory, A, would have been worth $25 to the vendor, while each additional ton of "warehouse capacity", B (i.e., each additional ton over the 200 maximum stipulated in the contract with the manufacturer), would have been worth $40. In other words, it would have been worthwhile for the vendor to pay up to $25 for an additional ton of inventory prior to period 1, and up to $40 per ton for relaxing the contractural limitation. Two remarks should be made in this connection, however. First, the extrapolation of increases in B and A cannot be made *ad infinitum*. There are definite limits to how far this process may be carried at the values of $25 and $40 per ton, respectively.[6] Second, these valuations (per unit change in the restrictions B and A) depend on the vendor's ability to programme optimally—i.e., to take maximum advantage of the opportunities provided by relaxing the restrictions. (This result follows from the dual theorem presented in Section 3. It will be noted that equality between the values for the dual and direct problems hold only at an optimum. Essentially, then, the process now being carried forward predicts the new optimum value for the dual and hence, also, for the direct problem as well (see reference 6). Because of the resulting equality between the values of the dual and direct problem, the optimum value of the dual can then be interpreted as the solution to the problem of valuing the fixed facilities. It yields the *minimum* total

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value (subject to the constraints set by market prices for products on which these facilities are to be employed) at which consideration should be given to sale or lease of the facilities instead of employing them productively. The minimum total value of these facilities is, obviously, the maximum value that can be obtained through their productive use by the owner himself.)

Less than optimal programmes will correspondingly reduce the value of the resulting programme. This point requires emphasis because of its effect on the stability of the physical programmes (e.g., tons produced and sold) as well as on the dollar values achieved. Optimal programmes are usually quite stable and unresponsive to relatively small changes in prices. (That is, even under altered prices the same physical programme remains the best that can be done in terms of a, perhaps, reduced or increased net return.) Non-optimal programmes are usually quite unstable and responsive to any change in conditions. Indeed, even when conditions do not alter, a recalculation may yield an improvement in net returns to the point where it becomes desirable to alter the previously decided upon programme. For an analysis of the mathematical reasons for these results, see reference 6. The authors have frequently encountered these phenomena in practice, but they are grateful to Dr. Donald Young, of General Mills, Inc., for stressing their importance.

A somewhat sharper question may be answered by rewriting the dual functional in expanded form:

$$\begin{aligned} E &= (B-A)t_1+(B-A)t_2+(B-A)t_3+(B-A)t_4+(B-A)t_5 \\ &\quad +Au_1+Au_2+Au_3+Au_4+Au_5 \\ &= (B-A)(p_2-c_1)+(B-A)(p_3-c_2)+(B-A)(c_4-c_3) \\ &\quad +(B-A)(p_5-c_4)+(B-A)\times 0+A\times 0 \\ &\quad +A(p_2-c_2)+A\times 0+Ap_5 \\ &= 10(B-A)+5(B-A)+10(B-A)+15(B-A)+0(B-A) \\ &\quad +0A+10A+5A+0A+50A. \end{aligned}$$

This version of the functional admits of answers to the question of valuing an alteration of the restriction during any one period while leaving the other periods unaffected. Thus an increase of "warehouse capacity" by one unit in the first period only would be worth \$10. During the second period it would be worth \$5, and so on. If this increase were allowed to obtain for the first two periods, the present optimum of \$10,500 could be increased to \$10,515. By cumulatively summing the values in this fashion an evaluation

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of relaxation in the restrictions over any sequence of periods may be obtained. It is perhaps of interest to note that the valuation $t_3 = c_4 - c_3 = \$10$ arises because the sale is made in period 5. Hence an extra ton allowance at this time allows the vendor to take advantage of the unit cost $c_3 = \$25$. His only other alternative would be to acquire one ton at $c_4 = \$35$ for sale in period 5.

Of course, it makes no sense to inquire into the effects of altering a "beginning inventory" which assumes one value in the opening period and a different value in one or more subsequent periods.* The effects of altering the opening inventory are twofold. On the one hand it reduces the quantity that may be purchased in that period for subsequent sale (as can be seen by the terms $t_k = p_k - c_{k-1}$ opposite the $B-A$, above); on the other hand it increases the quantity that may be sold at the price prevailing in that particular period and provides a "free" gift of goods which would otherwise have to be purchased at the relevant time (as can be seen by the terms $u_k = p_k - c_k$ opposite the A's). The effects of altering the opening inventory there have to be "chased down" through all the relevant trading periods. After this is done and appropriate cancellations are made, the single result $Ac_1 = A \times \$25$ will be secured, as already noted. An alteration (per ton) in opening inventory will, if used optimally, provide a \$25 increment to total profit.† This rise in total profit from \$10,500 to \$10,525 will occur if the vendor holds this stock of opening inventory through period 1, instead of selling at the prevailing offer price of \$20. He will then purchase only 99 tons instead of 100 (because of the limit set by $B-A$) at a price of \$25 in the first period, and dispose of his entire holding (101 tons of opening inventory plus 99 tons of period 1 purchases) at a price of \$35 per ton in period 2. His saving will therefore amount to \$25, and this will be the value of an additional ton of opening inventory to him, rather than the \$35 per ton disposal price, as might be assumed if the full range of his alternatives (and restrictions) were not considered. Stated differently, it would be worth his while to acquire an extra ton of beginning inventory only if the price were \$25 per ton or less.

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* Presumably some sense might be given to this question by assuming a gift of items of inventory (perhaps contingently) in subsequent periods. Such phenomena are, however, of little general interest and will not be pursued further.

† This case is of interest because of the double role that the initial inventory plays. An increase in inventory loosens the selling constraint and tightens the buying constraint. It is considerably harder in more complex cases to chase down and net out these opposite effects.

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10. *Multi-product Trading*

A question arises about modes of generalizing the preceding analysis and examples. The number of time periods to be considered may be expanded without introducing any novelty or difficulty. Variations in inventory or warehouse capacity easily fall into the suggested approach and inclusion of multiple warehouses simply requires an iteration of the model that has already been developed.

The means for effecting extensions in other directions are not so readily apparent. The remaining parts of this paper will therefore deal with multiple-product considerations and price alterations associated with the amount of trading.

This section will deal with extensions to multiple-product considerations. A distinction should be made, at the outset, between commodities which are traded independently of one another and commodities which are interdependent. A simple case of the former type occurs when each commodity is stored in a different warehouse and is sold in a different market. The preceding analysis then applies to each commodity considered separately. A simple case of interdependence occurs when the commodities compete for warehouse space. It is this case which will be considered.*

The inequalities which are diagrammatically displayed in the diagram (18) provide a representation for this case, which is obviously a generalization of (6). The capacity, B, of the warehouse remains as before, but separate-product designations must now be introduced.† This can be done as follows:

Let A_s = beginning inventory of s^{th} commodity,

x_{si} = amount purchased of s^{th} commodity in i^{th} period,

y_{si} = amount sold of s^{th} commodity in i^{th} period,

c_{si} = price charged per unit purchased for s^{th} commodity in i^{th} period,

---

* "Tie-in" sales or purchases (in which one product is sold to a customer in a certain amount only if he buys another product in a stipulated proportion) and other such relations, can be introduced, it is believed, without difficulty.

† The discerning reader, who has had some experience with linear programming, will note that this model has the classic structure of the so-called transportation problem of linear programming except that the ± 1's of typical transportation models are replaced by sub-matrices in place of the scalar elements (see references 4 and 12). Interpreted in these terms the model portrayed in (18) represents a generalized transportation model with one origin (the warehouse limitations) and several destinations. Several warehouses would repeat the row echelons (as sub-matrices of zeros and ones) and the different commodities would repeat the diagonal echelons in characteristic form.

p_{si} = price received per unit sold of the s^{th} commodity in the i^{th} period,

$$\text{for } s = 1, 2, \ldots, m;\ i = 1, 2, \ldots, n. \tag{17}$$

Then $$B - \sum_{s=1}^{m} A_s$$

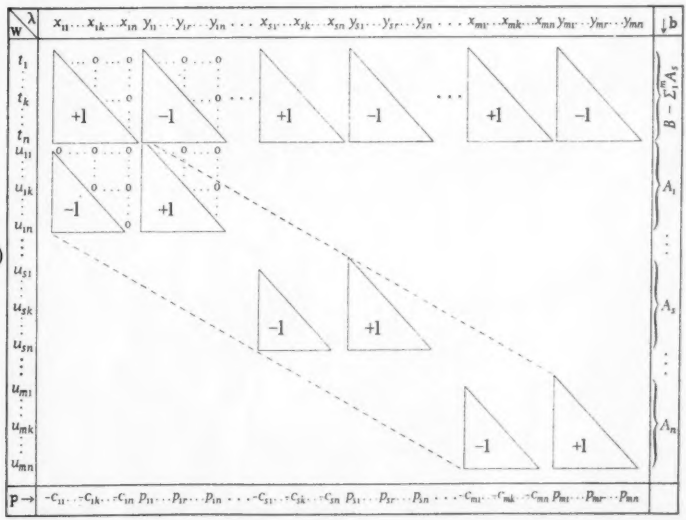
represents the amount of warehouse space initially available. All quantities are assumed to have been reduced to equivalent units, such as tons, cubic feet of warehouse space, or other appropriate measures. Thus

$$\sum_{s=1}^{m} A_s = A$$

is stated in the same units as B—as in the single-product case.

With this notation the dual and direct problems may be portrayed schematically as in (18). It is worth noting that the four

(18)



triangles in the upper left-hand corner of the diagram then represent the special single-commodity case portrayed in (6). Each quadruplet of triangles represents a repetition of this pattern with the two upper triangles (in the first row) tying all commodities to a common warehouse; the two lower triangles portray the selling constraint so that the amount of the s^{th} product sold in the i^{th} period cannot

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exceed the amount of that commodity which is available at the end of period $(i-1)$. Stated differently, the selling constraints reflect the fact that the different products do not interact on the sales side. It should be clear that market inter-relations can be portrayed by inserting triangles of the indicated kind in the relevant portions of the diagram and altering the relevant restrictions.

The procedures developed in the preceding sections can now be extended to encompass this problem. It will be noted that the t's are exactly the same as before with nothing essentially new. Double subscripts are necessary only for the u's in the dual formulation. The constraints may therefore be set forth* immediately as

$$\begin{aligned} &\sum_{i=k}^{n} t_i - \sum_{i=k+1}^{n} u_{si} \geq -c_{sk} && s = 1, \ldots, m \\ &-\sum_{i=r}^{n} t_i + \sum_{i=r}^{n} u_{si} \geq p_{sr} && r, k = 1, \ldots, n \end{aligned} \tag{19}$$

with, of course,

$$t_1, u_{si} \geq 0. \tag{20}$$

The dual functional, to be minimized, is then

$$(B - \sum_{s=1}^{m} A_s) \sum_{i=1}^{n} t_i + \sum_{s=1}^{m} A_s \sum_{i=1}^{n} u_{si}, \tag{21}$$

where

$$\sum_{i=1}^{n} u_{si} = U_{s1}.$$

Proceeding as before, the definitions

$$\begin{aligned} T_k &\equiv \sum_{i=k}^{n} t_i \qquad k = 1, \ldots, n \\ U_{sk} &\equiv \sum_{i=k}^{n} u_{si} \qquad s = 1, \ldots, m \end{aligned} \tag{22}$$

are introduced. Only the cumulative sums are considered in order to take advantage of the resulting monotonicity properties of the T's and U's. The (dual) problem may then be restated by replacing (13) by the following:

Minimize $\qquad E \equiv (B - \sum_{s=1}^{m} A_s) T_1 + \sum_{s=1}^{m} A_s U_{s1}$

* Cf. (7), (8), (9) and (10).

subject to:

$$
\begin{aligned}
&\text{(a)} \quad T_k \geqslant \max_{s=1,\ldots,m} (U_{s(k+1)} - c_{sk}),\\
&\text{(b)} \quad T_n \geqslant \max_{s=1,\ldots,m} (-c_{sn}),\\
&\text{(c)} \quad T_{r-1} \geqslant T_r,\\
&\text{(d)} \quad U_{sr} \geqslant T_r + p_{sr},\\
&\text{(e)} \quad U_{s(r-1)} \geqslant U_{sr},\\
&\text{(f)} \quad T_r, U_{sr} \geqslant 0,
\end{aligned}
\tag{23}
$$

for $k = 1, 2, \ldots, (n-1)$,

$s = 1, 2, \ldots, m$,

$r = 1, 2, \ldots, n$,

with $T_0 \equiv T_1, U_{s0} \equiv U_{s1}$.

The computational algorithm again takes the form of a series of alternations, determining first T_n from (b) or (f), then U_{sn}, for $s = 1, 2, \ldots, m$, from (d) or (f). Next the value of T_{n-1} is secured from (a) or (c) and then the value of $U_{s(n-1)}$ is secured from (d) and (e) for $s = 1, 2, \ldots, m$. In each case the minimum which can be secured is set by the maximum of the values appearing in the right-hand member of the relevant set of restrictions.

The alternation is pursued stage by stage until T_1 and the U_{s1}'s are secured. Insertion of these values in E yields the minimum

TABLE V

PURCHASE AND SALE PRICE QUOTATIONS FOR TWO PRODUCTS

$B = 200$ tons*

Period	Product 1: $A_1 = 100$ tons		Product 2: $A_2 = 25$ tons	
	Purchase Price	Selling Price	Purchase Price	Selling Price
1	$25 = c_{11}$	$20 = p_{11}$	$65 = c_{21}$	$50 = p_{21}$
2	$25 = c_{12}$	$35 = p_{12}$	$25 = c_{22}$	$35 = p_{22}$
3	$25 = c_{13}$	$30 = p_{13}$	$25 = c_{23}$	$25 = p_{23}$
4	$35 = c_{14}$	$25 = p_{14}$	$35 = c_{24}$	$40 = p_{24}$
5	$45 = c_{15}$	$50 = p_{15}$	$45 = c_{25}$	$55 = p_{25}$

* All quotations are in \$/ton or other "equivalent" units.

value for the dual problem, and hence (by the duality theorem) the maximum value for the direct problem. Application of the regrouping principle then provides a means for delineating a (direct) programme which achieves this optimal value. There may be other

programmes which are equally good, but there will be none which are better. See reference 6 for a discussion of means for locating such alternate optima. It is perhaps worth noting, also, that the dual variables are also available, by the above procedure just as in the uni-product case, to obtain valuations of the restrictions.

A simple extension of the example set forth in Section 8 may be used to fix ideas. Consider the array of prices and opening inventories for two products as set forth in Table I. The limitation, $B = 200$ tons, will be assumed to apply to both products, i.e., the manufacturer limits the vendor to a total "credit" of 200 tons outstanding in any period. The result is shown in Table V.

Applying the algorithm to the formulas (23) the results in Table VI are obtained. First $T_5 = 0$ is secured from the greater of the two quantities, (b) $T_n \geqslant \max_{s=1,2}(-c_{s5}) = -25$ or (f) $T_5 \geqslant 0$. Then the value $T_5 = 0$ is inserted in (d) to obtain the values $U_{15} \geqslant T_5 + p_{15} = 0 + 50$ and $U_{25} \geqslant T_5 + p_{25} = 0 + 55$. Since both results are positive, the non-negativity requirement (f), $U_{sr} \geqslant 0$, is ignored. Both of the values so obtained, however, must be carried forward and inserted in (a), as is done in the second row of the Table, since the maximum difference between $U_{15} - c_{14} = 50 - 35 = 15$ and $U_{25} - c_{24} = 55 - 35 = 20$ must be obtained to determine the minimum permissible level of $T_4 = 20$. Of course, the monotonicity properties, $T_{r-1} \geqslant T_r$ and $U_{s(r-1)} \geqslant U_{sr}$, of the cumulative sums must also be satisfied as indicated in columns (ii) and (iv) of the Table. Thus $U_{14} = U_{15} = 50$ rather than $U_{14} = T_4 + p_{14} = 45$ becomes the relevant value for T_3, as indicated by the entry in row 2, column (iv).

Continuing in this manner, the values $T_1 = 50$, $U_{11} = 75$, and $U_{21} = 100$ (which are necessary for determining the minimum value of the dual functional) are secured:*

$$\begin{aligned} \min E &= (B - A_1 - A_2)T_1 + A_1 U_{11} + A_2 U_{21} \\ &= (200 - 100 - 25) \times 50 + 100 \times 75 + 25 \times 100 = \$13{,}750. \end{aligned}$$

This, then, is the value which can be secured by arranging the sales (and purchases) optimally under the conditions of the direct problem.

To determine this programme the above terms are rearranged as follows:

$$BT_1 + A_1(U_{11} - T_1) + A_2(U_{21} - T_1).$$

* The maximum values for the T's to be carried over in determining the succeeding U's are indicated by the bold figures in column (i).

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Referring to the formulas listed in Table VI, the values

$$T_1 = p_{25} - c_{24} + p_{24} - c_{23} + p_{13} - c_{12} + p_{12} - c_{11},$$
$$U_{11} = p_{25} - c_{24} + p_{24} - c_{23} + p_{13} - c_{12} + p_{12},$$
$$U_{21} = p_{25} - c_{24} + p_{24} - c_{23} + p_{13} - c_{12} + p_{12} - c_{11} + p_{21},$$

are secured. After substituting and applying the regrouping principle, the following results are obtained:

$$\min E = \max \pi = A_2 p_{21} + B p_{12} + B p_{13} + B p_{24} + B p_{25} - (B - A_1) c_{11} - B c_{12} - B c_{23} - B c_{24}.$$

The instructions for trading are obvious from the symbols and subscripts. The programme is shown in Table VII.

11. *Multi-part Pricing*

Attention will now be directed to price variations. Two cases may be distinguished: (1) Sometimes the possible range of prices is unknown, so that interest attaches to the consequences associated with potential price discrepancies. (2) In some cases the price which obtains depends on the quantity sold (or bought).

Insofar as the first case requires, *ex ante*, the introduction of probabilistic considerations, it will not be dealt with here. G. B. Dantzig[10] gives a description of methods for dealing with probabilistic elements in the functional, and A. Charnes, W. W. Cooper and G. H. Symonds[8] discuss a case involving probabilistic functionals and probabilistic constraints. On occasion a great deal can be accomplished by studying the potential range of prices in order to ascertain the levels at which programme changes will be necessary. Parametric programming offers one means for making such determinations, the relations between the dual and direct problems offer another. Just as the dual functional may be used to evaluate changes in the restrictions, so the direct functional may be used to evaluate changes in the prices. These are aspects of the general problem of sensitivity analysis which comprehend changes in the matrix as well.

Reference to Table VI of Section 10 will show that variations in the c's or p's (purchase and sale prices) which do not alter subsequent U's or T's will not affect the physical programme. Such variations will alter only the level of dollar profits and leave the pattern of purchase and sale transactions unaffected.

An example may help clarify the meaning of this statement. Suppose that the purchase price per unit of Product 2 in period 3 (see Table V) is raised from $c_{23} = \$25$ to $\hat{c}_{23} = \$40$.* This change

* Alterations which do not reduce T_3 to a level below \$25 will, in general, have no effect, except possibly through the restrictions $T_{r-1} \geq T_r$ or $U_{s(r-1)} \geq U_{sr}$.

TABLE VI

APPLICATION OF ALGORITHM FOR TWO-PRODUCT CASE

Col./ row	(i) $T_k \geqslant \max_{s=1,\ldots,m} [U_{s(k+1)} - c_{sk}]$	(ii) $T_{r-1} \geqslant T_r$	(iii) $U_{sr} \geqslant T_r + p_{sr}$	(iv) $U_{s(r-1)} \geqslant U_{sr}$
1	$T_5 = 0$		$U_{15} = T_5 + p_{15} = 0+50 = 50$ $U_{25} = T_5 + p_{25} = 0+55 = 55$	
2	$T_4 = \begin{matrix} U_{15} - c_{14} = 50-35 = 15 \\ \mathbf{U_{25} - c_{24} = 55-35 = 20} \end{matrix}$		$U_{14} = T_4 + p_{14} = 20+25 = 45$ $U_{24} = T_4 + p_{24} = 20+40 = 60$	$U_{14} = U_{15} = 50$
3	$T_3 = \begin{matrix} U_{14} - c_{13} = 50-25 = 25 \\ \mathbf{U_{24} - c_{23} = 60-25 = 35} \end{matrix}$		$U_{13} = T_3 + p_{13} = 35+30 = 65$ $U_{23} = T_3 + p_{23} = 35+25 = 60$	
4	$T_2 = \begin{matrix} \mathbf{U_{13} - c_{12} = 65-25 = 40} \\ U_{23} - c_{22} = 60-25 = 35 \end{matrix}$		$U_{12} = T_2 + p_{12} = 40+35 = 75$ $U_{22} = T_2 + p_{22} = 40+35 = 75$	
5	$T_1 = \begin{matrix} \mathbf{U_{12} - c_{11} = 75-25 = 50} \\ U_{22} - c_{21} = 75-65 = 10 \end{matrix}$	$T_1 = T_2 = 40$	$U_{11} = T_1 + p_{11} = 50+20 = 70$ $U_{21} = T_1 + p_{21} = 50+50 = 100$	$U_{11} = U_{12} = 75$

TABLE VII

TRANSACTIONS FOR OPTIMUM PROGRAMME

Period	Transactions	Receipts ($)	Disbursements ($)	Difference ($)
1	Sell $A_2 = 25$ tons of Product 2 at \$50	1,250		
	Buy $(B - A_1) = 100$ tons of Product 1 at \$25		2,500	−1,250
2	Sell $B = 200$ tons of Product 1 at \$35	7,000		
	Buy $B = 200$ tons of Product 1 at \$25		5,000	2,000
3	Sell $B = 200$ tons of Product 1 at \$30	6,000		
	Buy $B = 200$ tons of Product 2 at \$25		5,000	1,000
4	Sell $B = 200$ tons of Product 2 at \$40	8,000		
	Buy $B = 200$ tons of Product 2 at \$35		7,000	1,000
5	Sell $B = 200$ tons of Product 2 at \$55	11,000	—	11,000
	Total	33,250	19,500	13,750

effects the choice of T_3 (i.e., the bold element in row 3, column (i) of Table VI) and of U_{13} and U_{23}. Hence a programme change is indicated if a price variation of this magnitude occurs.

To determine the effect of this price change it is not necessary to recalculate rows 1 and 2 of Table VI since c_{23} enters for the

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first time in row 3. The tabulations, commencing with row 3, may therefore be listed as in Table VIII.

TABLE VIII

RETABULATION OF TABLE VI WITH $\hat{c}_{23} = \$40$ IN PLACE OF $c_{23} = \$25$

Col./ row		(i)	(ii)	(iii)	(iv)
3	$T_3 =$	$\mathbf{U_{14}-c_{13}=50-25=25}$ $U_{24}-\hat{c}_{23}=60-40=20$		$U_{13}=T_3+p_{13}=25+30=55$ $U_{23}=T_3+p_{23}=25+25=50$	$U_{23}=U_{24}=60$
4	$T_2 =$	$U_{13}-c_{12}=55-25=30$ $\mathbf{U_{23}-c_{22}=60-25=35}$*		$U_{12}=T_2+p_{12}=35+35=70$ $U_{22}=T_2+p_{22}=35+35=70$	
5	$T_1 =$	$\mathbf{U_{12}-c_{11}=70-25=45}$ $U_{22}-c_{21}=70-65=5$	$T_1=T_2=35$	$U_{11}=T_1+p_{11}=45+20=65$ $U_{21}=T_1+p_{21}=45+50=95$	$U_{11}=U_{12}=70$

* $U_{23}=U_{24}=p_{25}-c_{24}+p_{24}$.

Substituting these results in the dual functional, the value

$$\begin{aligned}\min E &= (B-A_1-A_2)T_1+A_1U_{11}+A_2U_{21}\\ &= 75\times 45+100\times 75+25\times 95 = \$12{,}750\end{aligned}$$

is obtained. Optimal "profits" (i.e., net returns exclusive of initial inventory costs) would drop from \$13,750 to \$12,750 if the unit purchase price of Product 2 in period 3 were raised by \$15 a ton—i.e., from $c_{23} = \$25$ to $\hat{c}_{23} = \$40$.

The reduction in profits is held to only \$1,000 (rather than the full $\$15\times 200 = \$3{,}000$) because the vendor can (and should) alter his programme in response to such a variation in his quoted purchase price. To find the optimal programme associated with the level of profit, the regrouping principle need only be applied. First the T's and U's are restated in terms of the p's and c's by reference to Tables VI and VIII:

$$\begin{aligned}T_5 &= 0,\\ T_4 &= p_{25}-c_{24},\\ T_3 &= p_{25}-c_{24}+p_{14}-c_{13},\\ T_2 &= p_{25}-c_{24}+p_{24}-c_{22},\\ T_1 &= p_{25}-c_{24}+p_{24}-c_{22}+p_{12}-c_{11},\\ U_{11} &= p_{25}-c_{24}+p_{24}-c_{22}+p_{12},\\ U_{21} &= p_{25}-c_{24}+p_{24}-c_{22}+p_{12}-c_{11}+p_{21}.\end{aligned}$$

The data are then regrouped to yield

$$\begin{aligned}\min E &= BT_1 + A_1(U_{11} - T_1) + A_2(U_{21} - T_1) \\ &= p_{21}A_2 - (B - A_1)c_{11} + Bp_{12} - Bc_{22} + Bp_{24} - Bc_{24} \\ &\quad + Bp_{25} = \max \pi.\end{aligned}$$

Direct interpretation of the subscripts and reference to Table V yield the quantitative results indicated in Table IX.*

TABLE IX

TRANSACTIONS FOR OPTIMUM PROGRAMMES WITH $\hat{c}_{23} = \$40$ IN PLACE OF $c_{23} = \$25$

Period	Transactions	Receipts ($)	Disbursements ($)	Difference ($)
1	Sell $A_2 = 25$ tons of Product 2 at \$50	1,250		
	Buy $(B - A_1) = 100$ tons of Product 1 at \$25		2,500	−1,250
2	Sell $B = 200$ tons of Product 1 at \$35	7,000		
	Buy $B = 200$ tons of Product 2 at \$25		5,000	2,000
3	No transaction	0	0	0
4	Sell $B = 200$ tons of Product 2 at \$40	8,000		
	Buy $B = 200$ tons of Product 2 at \$35		7,000	1,000
5	Sell $B = 200$ tons of Product 2 at \$55	11,000	0	11,000
	Total	27,250	14,500	12,750

Clearly this kind of analysis may be extended to determine critical levels at which price variations will have programme consequences. Separate managerial decisions may then be made, on the basis of such *ex post* evaluations, to determine whether the likelihood of such price variations is sufficiently serious to warrant programme adjustments. The level of profit reduction which attaches to such programme alterations then provides a measure of the "risk premium" which management bears in such cases.

* Note that this programme is also available for the problem stated in Section 10. It is perhaps also of interest to note in comparing the programmes in Tables VII and IX that the latter yields a larger return on cost (exclusive of inventory cost), viz.:

$$\frac{12,750}{14,500} = .88 \text{ vs. } \frac{13,750}{19,500} = .70.$$

This should occasion no surprise. The objective here is to maximize absolute profit. In general, maximization of absolute returns, return on cost, investment, proprietary net worth, etc., do not yield the same result. See Lutz[16] for further development.

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Still other modes of exploring the consequences of data alteration (and uncertainty) might be indicated. Instead, attention will be turned to the second of the two problems mentioned in the opening paragraph of this section—the case in which prices vary as a function of the quantity purchased or sold. Only price variations on the selling side will be discussed. Moreover, only one specimen, which might be called multi-part pricing, will be covered. Such pricing occurs when it is possible to divide the customers into two (or more) groups and charge different prices to each, including cases, as in public utilities, where customers receive more favourable rates on purchases beyond a specified quantity. That is, pricing is discriminatory. Different prices are charged to different groups of customers for the same product at the same time.

To make matters more concrete, the vendor of the preceding sections may be viewed as having two groups of customers available in each period. The first, or preferred group, from his viewpoint, is willing to pay a price of $\$p$ per unit up to $D \leqslant 200$ units in any period. Additional sales, if any are to be made during the period, must then be at an offer price of $\$q \leqslant \$p$ per unit to the second group of customers.

Standard methods have been devised for handling this type of non-linearity which may be adapted to the warehousing model[5,6]. In effect, the method consists of altering the problem by introducing extra variables. The altered problem (which is linear) has, however, the same optimum as the original problem—i.e., the optimum programmes and profit levels are the same for both problems.

Suppose, for example, that for each of the m products in each of the n periods only the amounts D_{sr} can be sold at the preferred price $p_{sr}, s = 1, 2, \ldots, m; r = 1, 2, \ldots, n$. Sales of the s^{th} product in the r^{th} period which exceed the quantity D_{sr} can be executed only at a price $q_{sr} \leqslant p_{sr}$. It will be assumed that the manufacturer is willing to sell to the vendor, within the limits previously stated, at a uniform price of $\$c_{sk}$ per unit.

The net accumulated amounts on hand at the start of any period (the accumulated purchases x, plus the initial inventory A, minus the accumulated sales y) will bring $\$p$ per unit provided no more than D units are sold. Extra sales bring a price of only $\$q \leqslant \$p$ per unit. Thus the portion of the functional which is associated with the x's and y's remains unaltered. These variables still enter at $-\$c$ and $+\$p$ per unit respectively, but extra constraints must be introduced to limit the sales which may be executed at $\$p$ per unit. (Similar constraints would be employed if purchase rather than sales prices were allowed to vary in this manner.) Moreover,

new variables, z, are introduced into the analysis to represent the opportunity of additional sales at $\$q$ per unit in any period.

A mathematical representation of the constraints in the direct problem is as follows (cf. equations (1.1) ff. in Section 2):

$$\begin{aligned}
\sum_{s=1}^{m}\left(\sum_{k=1}^{i} x_{sk}-\sum_{r=1}^{i} y_{sr}-\sum_{r=1}^{i} z_{sr}\right) &\leqslant B-\sum_{s=1}^{m} A_s \\
-\sum_{k=1}^{(i-1)} x_{sk}+\sum_{r=1}^{i} y_{sr}+\sum_{r=1}^{i} z_{sr} &\leqslant A_s \\
y_{si} &\leqslant D_{si} \\
x_{sk}, y_{sr}, z_{sr}, y_{si}, &\geqslant 0 \\
i &= 1,2,\ldots,n \\
s &= 1,\ 2,\ \ldots,m
\end{aligned} \tag{24}$$

so that the acquisitions in any period are limited, as before, by the available warehouse space, $B-\sum_{s=1}^{m} A_s$, and the sales for any product during any period are limited to the amount of that product which is available at the start of the period, viz.:

$$y_{si}+z_{si}\leqslant A_s+\sum_{k=1}^{(i-1)}(x_{sk}-y_{sk}-z_{sk}),$$

or,

$$-\sum_{k=1}^{(i-1)} x_{sk}+\sum_{r=1}^{i} y_{sr}\quad+\sum_{r=1}^{i} z_{sr}\leqslant A_s.$$

The variables y_{si} and z_{si} denote, respectively, the quantities sold at prices p_{si} and q_{si}; hence the y_{si} are limited to at most D_{si} in any period, $i = 1,2,\ldots,n$, since that is the maximum quantity for which customers will pay p_{si} per unit during that period. The functional to be maximized can then be written as

$$\pi=\sum_{s=1}^{m}\left[\sum_{k=1}^{n}(y_{sk}p_{sk}+z_{sk}q_{sk}-x_{sk}c_{sk})\right] \tag{25}$$

where, of course, maximization is to be undertaken subject to the constraints (24). Clearly the maximization process will guarantee selection of the y_{si} over the z_{si} up to the allowable amount D_{si} since $p_{si}\geqslant q_{si}$.

The types of diagrammatic arrangements previously employed may again be used to portray the structure of the model and to provide a convenient means for writing the dual and direct problems. Such a diagram is presented in (26).

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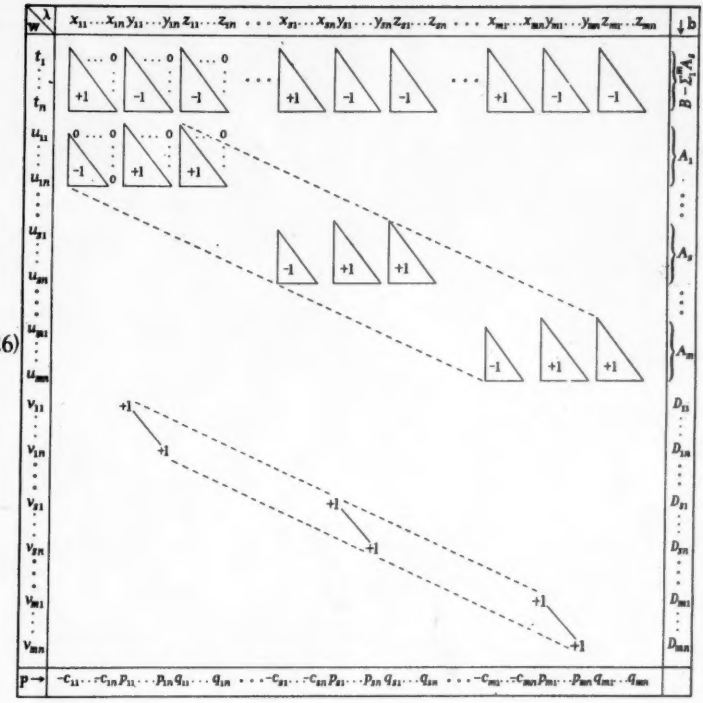
(26)



The dual restrictions may be obtained directly from the above diagram:

$$
\begin{aligned}
\sum_{i=k}^{n} ti - \sum_{i=k+1}^{n} u_{si} &\geqslant -c_{sk} \\
-\sum_{i=r}^{n} t_i + \sum_{i=r}^{n} u_{si} + v_{sr} &\geqslant p_{sr} \\
-\sum_{i=r}^{n} t_i + \sum_{i=r}^{n} u_{si} &\geqslant q_{sr} \qquad (27) \\
t_i, u_{si}, v_{sr} &\geqslant 0 \\
s &= 1, \ldots, m, \\
k &= 1, \ldots, n, \\
r &= 1, \ldots, n.
\end{aligned}
$$

The functional may be similarly formulated to yield

$$\min E = \left(B - \sum_{s=1}^{m} A_s\right) \sum_{i=1}^{n} t_i + \sum_{s=1}^{m} A_s \sum_{r=1}^{n} u_{sr} + \sum_{s=1}^{m} \sum_{r=1}^{n} D_{sr} v_{sr}$$

where

$$\left(B - \sum_{s=1}^{m} A_s\right), A_s, D_{sr}, (p_{sr} - q_{sr}) \geqslant 0.$$

Effecting a change of variables from t's and u's to T's and U's the functional and restrictions (27) may be stated more compactly and simply as

$$\min E = \left(B - \sum_{s=1}^{m} A_s\right) T_1 + \sum_{s=1}^{m} A_s U_{s1} + \sum_{s=1}^{m} \sum_{r=1}^{n} D_{sr} v_{sr},$$

subject to

$$\begin{aligned}
&\text{(a)} && T_k \geqslant \max_{s=1,\ldots,m} (U_{s(k+1)} - c_{sk}), \\
&\text{(b)} && T_n \geqslant \max_{s=1,\ldots,m} (-c_{sn}), \\
&\text{(c)} && T_{r-1} \geqslant T_r, r = 2, \ldots, n, \\
&\text{(d}_1\text{)} && U_{sr} \geqslant T_r + q_{sr}, \\
&\text{(d}_2\text{)} && v_{sr} + U_{sr} \geqslant T_r + p_{sr}, \\
&\text{(e)} && U_{s(r-1)} \geqslant U_{sr}, \\
&\text{(f)} && T_r, U_{sr}, v_{sr}, (p_{sr} - q_{sr}) \geqslant 0.
\end{aligned} \tag{28}$$

Comparisons between (28) and (23) are instructive. The first two terms in both functionals are the same. In both cases $\min E (= \max \pi)$ is achieved by making T_1 and U_{s1} $(s = 1, 2, \ldots, m)$ as small as possible. Starting with T_n, proceeding to U_{sn} and determining in successive alternations, T_r and U_{sr}, each value is made as small as the constraints will allow. At the final stage the smallest possible values of T_1 and U_{s1} are thus obtained. No interference is, so to speak, encountered. Attainment of the lowest admissible value of the T's and U's at each stage will never react by forcing a higher value at some later stage.

The terms involving v's in the functional and restrictions of (28) introduce a complication. Note, for example, that restriction (d_2) involves both v_{sr} and U_{sr}. Reference must therefore be made to their accompanying coefficients, A_s and D_{sr}, in the functional in deciding which variable should be lowered (possibly at the expense of raising the other). Moreover, unlike the T's and U's the v_{sr} appear in the functional for every time period, $r = 1, 2, \ldots, n$ and every product, $s = 1, 2, \ldots, m$. The associated coefficients, D_{sr}, must therefore be considered relative to one another as well as to the A_s.

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This complication is handled by conducting the calculations in two stages—an α and a β stage. The algorithm (29) is applied in the first or α stage in order to determine the T's, U's and v's in the same manner as previously. That is, values of T_k, U_{sk} and v_{sk} are determined by beginning in the last period and proceeding to the first. The procedure is then reversed. Noting that only U_{s1} enters into the functional whereas all v_{sk} do, the values of these terms are altered by commencing with the first period and proceeding to the last. The objective in this second or β stage is to lower the v_{sk} even at the expense of raising the corresponding U_{sk}. The compensating raising and lowering is carried out to the extent allowed by the restrictions, but never to an amount which will affect subsequent (earlier period) T_k's or U_{sk}'s.

These general instructions may be given more concrete meaning by setting forth the algorithm and applying it to a concrete example. For use in the first stage the algorithm is as follows [the symbol $\cdot)\cdot$ is used to indicate "such that"]:

(0) $$T_k = \max\left\{T_{k+1}, \max_{s=1,2,\ldots}[U_{s(k+1)} - c_{sk}]\right\}.$$

(1) For all s $\cdot)\cdot$

$$T_k + p_{sk} \leqslant U_{s(k+1)},$$

then $$v_{sk} = 0 \quad \text{and} \quad U_{sk} = U_{s(k+1)}.$$

(2) For all s $\cdot)\cdot$

$$T_k + p_{sk} > U_{s(k+1)} \quad \text{and} \quad A_s \geqslant D_{sk},$$

then $$v_{sk} = \min\{T_k + p_{sk} - U_{s(k+1)}, p_{sk} - q_{sk}\}$$

and $$U_{sk} = T_k + p_{sk} - v_{sk}.$$

(3) If $$\max_{s=1,2,\ldots}\{U_{s(k+1)} - c_{s(k-1)}, T_k + p_{sk} - c_{s(k-1)}\} = U_{s_1(k+1)} - c_{s_1(k-1)},$$ (29)

then if

(a) $A_s < D_{sk}$ then $$v_{sk} = 0 \quad \text{and} \quad U_{sk} = \max(T_k + p_{sk}, U_{s(k+1)}),$$

(b) $A_s \geqslant D_{sk}$ then $$U_{sk} = \max(T_{k1} + q_{sk}, U_{s(k-1)})$$ and $$v_{sk} = \max(T_k + p_{sk} - U_{sk}, 0).$$

(4) If $$\max_{s=1,2,\ldots}\{U_{s(k+1)} - c_{s(k-1)}, T_k + p_{sk} - c_{s(k-1)}\} = T_k + p_{s_1 k} - c_{s_1(k-1)},$$

then if

(a) $B_s \equiv B - \sum_{l=1}^{m} A_l + A_{s_1} < D_{s_1 k}$ then $v_{s_1 k} = 0$

and $U_{s_1 k} = \max(T_k + p_{s_1 k}, U_{s_1(k+1)})$.

(b) $B_s \geqslant D_{s_1 k}$ then $v_{s_1 k}$

$= \min\{T_k + p_{s_1 k} - U_{s_1(k+1)}, p_{s_1 k} - q_{s_1 k}\}$

and $U_{s_1 k} = T_k + p_{s_1 k} - v_{s_1 k}$.

(5) If $T_k + p_{s_1 k} - v_{s_1 k} \geqslant \max_{s \neq s_1} \{U_{s(k+1)}, T_k + p_{sk}\}$,

then rules 3(a) and 3(b) apply in determining the remaining v_{sk} and U_{sk}.

(6) If

$$T_k + p_{s_1 k} - v_{s_1 k} < \max_{s \neq s_1} \{U_{s(k+1)}, T_k + p_{sk}\} = U_{s_1(k+1)},$$

then rules 3(a) and 3(b) apply in determining the remaining v_{sk} and U_{sk}.

If

$$T_k + p_{s_1 k} - v_{s_1 k} < \max_{s \neq s_1} \{U_{s(k+1)}, T_k + p_{sk}\} = T_k + p_{s_2 k},$$

then rule 4, with s_1 replaced by s_2, applies in determining the remaining $v_{s_2 k}$, $U_{s_2 k}$, and possibly all.

It must be remembered, however, that a second stage is required for the purpose of adjusting the v's downward, if this can be done without affecting subsequent T's and U's.

An Example

The data of Table V, Section 10, may be extended to provide an illustrative example:

Suppose the following changes are made: The purchase price of product 2 in period 3 is \$40 in place of the \$25 which was previously used. In the fifth period, a maximum of 20 tons of this product may be sold at the \$55 price. Additional sales may be effected at only \$45 per ton. The \$30 price for product 1 in period 3 can be obtained only on sales up to 150 tons, all additional sales during this period requiring execution at a price of \$25. Finally, up to 100 tons of product 1 may be sold at \$50 per unit in the fifth period. Additional sales must be priced at \$40 per ton. The relevant data are contained in Table X.

Table XI incorporates the results of applying the algorithm to this illustrative example and provides the detail necessary for securing the dual evaluation and applying the regrouping principle. Each period is divided into two sets of rows, α rows and β rows. The former are directed to the first stage of calculations; the latter (β rows) are directed to the second "pass" which is made only after

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the α stage of calculation is completed for all periods. It will be noted that no β row is provided for period 1 since T_1, U_{11} and U_{21} must never be altered at the β stage.

In the panel labelled product 1 the data under p_{1k}, $-c_{1(k-1)}$, A_1-D_{1k}, B_1-D_{1k}, and q_{1k} are entered directly from Table X. Only the signs of A_1-D_{1k} and B_1-D_{1k} are entered, since this suffices for the algorithm. That is, only the relations $A_1 \gtrless D_{1k}$ and $B_1 \lesseqgtr D_{1k}$ are required. An additional column $U_{1k}-c_{1(k-1)}$ is calculated when the value of U_{1k} is ascertained, since these values are needed in succeeding calculations—e.g., to determine the succeeding T at the α stage and the critical limits at the β stage. These critical limits are indicated by the figures printed in bold type which, it will be noted, are equal to the value of T which succeeds them. The same treatment is, of course, accorded to the corresponding columns of the panel under product 2.

TABLE X

DATA FOR MULTI-PART PRICING FOR TWO PRODUCTS

$B = 200$ tons*

	Product 1: $A_1 = 100$ tons				Product 2: $A_2 = 25$ tons			
Period	Purchase Price ($)	Premium Sales ($)	Selling Price Quantity limit (tons)	Other sales ($)	Purchase price ($)	Premium sales ($)	Selling Price Quantity limit (tons)	Other sales ($)
1	$25 = c_{11}$	$20 = p_{11}$	—	—	$65 = c_{21}$	$50 = p_{21}$	—	—
2	$25 = c_{12}$	$35 = p_{12}$	—	—	$25 = c_{22}$	$35 = p_{22}$	—	—
3	$25 = c_{13}$	$30 = p_{13}$	$150 = D_{13}$	$25 = q_{13}$	$40 = c_{23}$	$25 = p_{23}$	—	—
4	$35 = c_{14}$	$25 = p_{14}$	—	—	$35 = c_{24}$	$40 = p_{24}$	—	—
5	$45 = c_{15}$	$50 = p_{15}$	$100 = D_{15}$	$40 = q_{15}$	$45 = c_{25}$	$55 = p_{25}$	$20 = D_{25}$	$45 = q_{25}$

* Tons or other "equivalent" units.

The full array of (26) is obtained by setting $D_{sr} = B$ for the cases in which no explicit value is stated in the columns labelled "Quantity limit".

The algorithm is more complex to write than it is to apply, as a little practice will show. Consider, for example, the problem set forth in Table X. The value

$$T_5 = \max\{0, \max(-c_{sk})\} = 0$$

is immediately secured and entered in the value row of α under T_k.

To determine v_{15} and U_{15} note that $T_5+p_{15} > U_{16} \equiv 0$ and $A_1 \geqslant D_{15}$. Hence rule (2) applies and

$$v_{15} = \min\{T_5+p_{15}-U_{16}, p_{15}-q_{15}\} = p_{15}-q_{15} = 10,$$
$$U_{15} = T_5+p_{15}-v_{15} = 0+50-10 = 40.$$

These values are entered in their appropriate position under product 1 evaluations in Table XI. The value $U_{15}-c_{14} = 40-35 = 5$ is entered immediately thereafter.

U_{25} and v_{25} are determined in the same way. $T_5+p_{25} > U_2 \equiv 0$ and $A_2-D_{25} > 0$, so that rule (2) is again applied to yield

$$v_{25} = \min\{0+55+0, 55-45\} = p_{25}-q_{25} = 10,$$
$$U_{25} = T_5+p_{25}-v_{25} = 55-10 = 45.$$

Entering these values along with $U_{25}-c_{24} = 45-35 = 10$, the first row of Table XI is completed. (It should be noted that the non-negativity requirements are fulfilled.)

TABLE XI

APPLICATION OF MULTI-PRODUCT, MULTI-PART PRICING ALGORITHM

Product 1

	Period	T_k	p_{1k}	$-c_{1(k-1)}$	A_1-D_{1k}	B_1-D_{1k}	q_{1k}	v_{1k}	U_{1k}	$U_{1k}-c_{1(k-1)}$
5α	Value	0	50	−35	0	+	40	10	40	5
	Rule	(0)						(2)	(2)	
	Formula	$T_5=0$						$v_{15}=p_{15}-q_{15}$	$U_{15}=p_{15}-v_{15}$	
5β	Value							5	45	10
	Formula							$v_{15}=T_5+p_{15}-q_{25}+c_{24}-c_{14}$	$U_{15}=U_{25}-c_{24}+c_{14}$	$U_{15}-c_{14}=U_{25}-c_{24}$
4α	Value	10	25	−25	—	—		0	40	**15**
	Rule	(0)						(1)	(1)	
	Formula	$T_4=U_{25}-c_{24}$							$U_{14}=U_{15}$	
4β	Value								45	**20**
	Formula								$U_{14}=U_{23}-c_{22}+c_{12}-p_{13}+c_{13}$	$U_{14}-c_{13}=U_{23}-c_{22}+c_{12}-p_{13}$
3α	Value	15	30	−25	—	+	25	5	40	15
	Rule	(0)						(6.2) and (4b)	(6.2) and (4b)	
	Formula	$T_3=U_{14}-c_{13}$						$v_{13}=p_{13}-q_{13}$	$U_{13}=T_3+p_{13}-v_{13}$	
3β	Value	20						0	50	25
	Formula	$T_3=U_{23}-c_{22}+c_{12}-p_{13}$						$v_{13}\geq T_3+p_{13}-v_{23}+c_{22}-c_{12}$	$U_{13}=U_{23}-c_{22}+c_{12}$	$U_{13}-c_{12}=U_{23}-c_{22}$
2α	Value	25	35	−25	—	—		0	60	**35**
	Rule	(0)						(4a)	(4a)	
	Formula	$T_2=U_{23}-c_{22}$							$U_{12}=T_2+p_{12}$	
2β	Value									
	Formula									
1α	Value	35	20		—	—		0	60	
	Rule	(0)						(1)	(1)	
	Formula	$T_1=U_{12}-c_{11}$							$U_{11}=U_{12}$	

Product 2

	Period	T_k	p_{2k}	$-c_{2(k-1)}$	A_2-D_{2k}	B_2-D_{2k}	q_{2k}	v_{2k}	U_{2k}	$U_{2k}-c_{2(k-1)}$
5γ	Value	0	55	−35	+	+	45	10	45	**10**
	Rule	(0)						(2)	(2)	
	Formula	$T_5=0$						$v_{25}=p_{23}-q_{25}$	$U_{25}=p_{25}-v_{25}$	
5β	Value									
	Formula									
4γ	Value	10	40	−40	−	−		0	50	10
	Rule	(0)						(6.2) and (4a)	(6.2) and (4a)	
	Formula	$T_4=U_{25}-c_{24}$							$U_{24}=T_4+p_{24}$	
4β	Value									
	Formula									
3α	Value	15	25	−25	−	−		0	50	**25**
	Rule	(0)						(1)	(1)	
	Formula	$T_3=U_{14}-c_{13}$							$U_{23}=U_{24}$	
3β	Value	20								
	Formula	$T_3=U_{23}-c_{22}+c_{12}-p_{13}$								
2α	Value	25	35	−65	−	−		0	60	−5
	Rule	(0)						(5) and (3a)	(5) and (3a)	
	Formula	$T_2=U_{23}-c_{22}$							$U_{22}=T_2+p_{22}$	
2β	Value								85	20
	Formula								$U_{22}=U_{21}$	
1α	Value	35	50		−	−		0	85	
	Rule	(0)						(6.2) and (4a)	(6.2) and (4a)	
	Formula	$T_1=U_{12}-c_{11}$							$U_{21}=T_1+p_{21}$	

Note: The following values are obtained from Table X:

$B=200$ $\quad A_1=100$ $\quad A_2=25$ $\quad B_1=B-A_2=175$ $\quad B_2=B-A_1=100$ $\quad D_{13}=150$ $\quad D_{15}=100$ $\quad D_{25}=20$

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The two rows under 5β are skipped since these are reserved for entries in the second "pass". Since no market restriction applies to the amount that can be sold at prices p_{14} and p_{24} in these periods, the values $D_{14} = D_{24} = B = 200$ apply, i.e., the warehouse capacity alone limits the amount that can be sold. (See note under Table X and the diagrammatic portrayal in (26).) All four differences are therefore scored negatively in this row.

Rule (0) applies for determining T_4. Noting from row 5α that

$$U_{25} - c_{24} = 10 > U_{15} - c_{14} = 5 > T_5 = 0,$$

the value $$T_4 = \max\left\{T_5, \max_{s=1,2}[U_{s5} - c_{s4}]\right\} = U_{25} - c_{24} = 10$$

is entered under T_k in 4α. Rule (1) applies again in determining $v_{14} = 0$ and $U_{14} = U_{15} = 40$ since

$$T_4 + p_{14} = 10 + 25 < U_{15} = 40.$$

For $s = 2$, however, the inequality is reversed, viz.,

$$T_4 + p_{24} = 10 + 40 > U_{25} = 45.$$

Therefore rule (1) cannot be used to determine v_{24} and U_{24}. Moreover, $A_2 = 25 < D_{24} = 200$, so rule (2) is eliminated. An $s_1 = 1$ has already been determined, so rules (3) and (4) are skipped in favour of a choice between rules (5) and (6). Noting

$$T_4 + p_{14} - v_{14} < \max_{s \neq 1}\{U_{25}, T_4 + p_{24}\} = T_4 + p_{24} = 50,$$

rule (6) is found to be applicable. According to the second instruction noted thereunder, recourse is had to rule (4). Since

$$\max_{s \neq 1}\{U_{25} - c_{23}, T_4 + p_{24} - c_{23}\} = T_4 + p_{24} - c_{23} = 10$$

and $$B_2 = B - A_1 = 100 < D_{24} = 200,$$

rule (4b) is relevant. Applying this rule yields

$$v_{24} = 0,$$
$$U_{24} = \max\{T_4 + p_{24}, U_{25}\} = T_4 + p_{24} = 50,$$

as noted in the product 2 panel of 4α. The row is completed by calculating $U_{14} - c_{13} = 40 - 25 = 15$ and $U_{24} - c_{23} = 50 - 40 = 10$.

Using rule (0), as usual, in order to determine T_3, the value

$$T_3 = \max\left\{T_4, \max_{s=1,2}[U_{s4} - c_{s3}]\right\} = U_{14} - c_{13} = 15$$

is inserted under column T_k. Now $T_3 + p_{23} \leqslant U_{24} = 50$, so $v_{23} = 0$ and $U_{23} = U_{24} = 50$, by rule (1), and these values appear under the product 2 panel of row 3α. However, $T_3 + p_{13} > U_{14}$, so rule (1) cannot be used to determine v_{13} and U_{13}. Moreover, $A_1 = 100 < D_{13} = 150$ (as indicated by the negative score in row 3α) so neither rule (1) nor rule (2) can be used. Note now that $s_1 = 2$ (the second product) has already been specified along with $v_{s_1 3} = v_{23} = 0$ so rules (3) and (4) are skipped in favour of rules (5) and (6). To choose between these last two rules the calculation

$$40 = T_3 + p_{23} - v_{23} < \max_{s \neq 2}\{U_{s4}, T_3 + p_{s3}\} = T_3 + p_{s_3 3} = T_3 + p_{13}$$
$$= 15 + 30 = 45$$

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shows that the second part of rule (6) is the relevant one. Following the instruction therein noted, rule (4) is brought into play by determining

$$\max_{s=1}\{U_{14}-c_{12}, T_3+p_{13}-c_{12}\} = T_3+p_{13}-c_{12}.$$

Noting that

$$B_1 \equiv B-A_1-A_2+A_1 = 200-25 = 175 \geqslant D_{13} = 150,$$

rule (4b) is selected to yield

$$v_{13} = \min\{T_3+p_{13}-U_{14}, p_{13}-q_{13}\} = p_{13}-q_{13} = 5,$$
$$U_{13} = T_3+p_{13}-v_{13} = 40,$$

and these values appear in row 3α of the table.*

Rule (0) is again applied to yield

$$T_2 = \max\left\{T_3, \max_{s=1,2}[U_{s3}-c_{s2}]\right\} = U_{23}-c_{22} = 25.$$

The U's and v's for the period must now be determined. Since $T_2+p_{s2} > U_{s3}$ and $A_s < D_{s2} = B$ for $s = 1, 2$, neither rules (1) nor (2) can be used. Moreover, an s_1 has not been designated so attention is directed to a choice between rules (3) and (4). Calculating

$$\max_{s=1,2}\{U_{s3}-c_{s1}, T_2+p_{s2}-c_{s1}\} = T_2+p_{s_12}-c_{s_12} = T_2+p_{12}-c_{11} = 35,$$

rule (3) is eliminated from consideration at this point. Noting

$$B_s < D_{s_12} = D_{12} = B = 200,$$

the values
$$v_{s_12} = v_{12} = 0,$$
$$U_{s_12} = U_{12} = \max\{T_2+p_{12}, U_{13}\} = T_2+p_{12} = 60$$

are ascertained by reference to (4a) and entered in the first panel of the table.

With values v_{12} and U_{12} now determined and a specified $s_1 = 1$ available, the choice of rules for determining v_{22} and U_{22} evidently reduces to (5) or (6).

Since
$$T_2+p_{12}-v_{12} \geqslant \max_{s\neq s_1}\{U_{23}, T_2+p_{22}\} = T_2+p_{22} = 60,$$

rule (5) is designated. The instruction under this rule requires reversion to rules (a) or (b) under (3). Since $A_s < D_{s2}$, part (a) of (3) applies:

$$v_{22} = 0,$$
$$U_{22} = \max(T_2+p_{22}, U_{23}) = T_2+p_{22} = 60.$$

To obtain

$$T_1 = \max\left\{T_2, \max_{s=1,2}[U_{s2}-c_{s1}]\right\} = U_{12}-c_{11} = 35,$$

rule (0) is used. The values†

$$v_{11} = 0$$
$$U_{11} = U_{12} = 60$$

---

* It will be noted that $T_3+p_{13}-U_{14} = p_{13}-q_{13} = 5$. These ties are associated with the possibility of alternative programmes which yield the same optimum values. This subject will not be discussed here (see reference 6). No difficulty need be experienced since either set may be arbitrarily chosen as yielding the correct values.

† Note the entries for $-c_{1(k-1)}$ and $-c_{2(k-1)}$ are omitted in this row since they have no meaning for $k = 1$.

are immediately determined by reference to rule (1) since $T_1+p_{11}<U_{12}$. With $s_1=1$ and

$$T_1+p_{11}-v_{11}<\max_{s\neq 1}\{U_{22},T_1+p_{21}\}=T_2+p_{21}=85$$

and $B_1<D_{21}=200$, rules (6) and (4a) are brought into play to yield

$$v_{21}=0,$$
$$U_{21}=\max(T_1+p_{21},U_{22})=T_1+p_{21}=85.$$

This completes the first or α stage of calculations. In this first "pass" the values of T_1 and U_{s1} which enter directly into the functional E of (28) have been minimally determined. Another pass is now made in order to ensure that the $v_{sk}, k=1,2,\ldots,n$, which also enter into the functional are in fact at their minimizing values. This is done by starting with period 1 at the bottom of Table XI and working back up to the top. Wherever possible the values v_{sk} are lowered (within the limit set by the non-negativity constraint, $v_{sk}\geqslant 0$) and the values U_{sk} raised. This lowering (and raising) is limited by the fact that it must not be carried to the point where the already attained minimizing values $T_1=35$, $U_{11}=60$, $U_{21}=85$ are affected.

Proceeding backwards from period 1 it is seen that the first critical value is encountered at $U_{12}-c_{11}=35$ under product 1. Raising this value will also raise T_1. On the other hand, $U_{22}=60$ may be altered to $U_{22}=85$ without affecting $U_{12}=85$. Any further rise in this value encounters restriction (28e): $U_{s(r-1)}\geqslant U_{sr}$.

Note that raising the value of U_{22} to 85 as indicated under U_{2k} in row 2β tends to relax restrictions on preceding values without affecting any of the values T_1, U_{11}, U_{21} which enter directly into the functional. In particular this increase may allow a lowering of a preceding v at the expense of raising a preceding U (or T) and thus provide a lower value of E. Hence U_{22} is raised to the maximum permissible extent even though no immediate advantage is offered by lowering $v_{22}=0$ which is already at the limit allowed by non-negativity.

The next critical value is encountered at $U_{23}-c_{22}=25$. Attention is therefore turned to $U_{13}-c_{12}=15$. Clearly this value may be raised to

$$U_{13}-c_{12}=U_{23}-c_{22}=25$$

or

$$U_{13}=U_{23}-c_{22}+c_{12}=50$$

without affecting any successor. The restriction being explored is

$$(d_2)\quad v_{sr}+U_{sr}\geqslant T_r+p_{sr}$$

in order to see whether a lowering of any v may be secured without raising any of the first period T's or U's. Setting

$$v_{13}\geqslant T_3+p_{13}-U_{23}+c_{22}-c_{12},$$

it is seen that v_{13} may be set equal to zero without wholly equating the two sides. Slack still remains for raising T_3 from 15 to 20 via

$$T_3=U_{23}-c_{22}+c_{12}-p_{13}=20.$$

All of these alterations are entered in rows 3β and a search is made for the next critical value at $U_{14}-c_{13}=15$. Note, however, that altering T_3 from

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15 to 20 in the preceding calculations has raised the critical level at the period 4 stage. By applying

$$U_{14}-c_{13}=T_3=U_{23}-c_{22}+c_{12}-p_{13}=20$$

the alterations appearing opposite 4β in the first panel are obtained. No alteration of v_{14} occurs because the value obtained at the α stage is already as low as the non-negativity restriction admits.

Nothing can be done with v_{24} or U_{24}. The former is already at its lowest admissible level and the constraint $U_{23}\geqslant U_{24}$ prevents any raising of the latter.

The next critical value is located at $U_{25}-c_{24}=10$. Turning therefore to the first panel and setting

$$U_{15}-c_{14}=U_{25}-c_{24}=10,$$

it is seen that U_{15} may be raised from 40 to 45 by means of the relation

$$U_{15}=U_{25}-c_{24}+c_{14}=45.$$

Moreover, the preceding alteration which raised U_{14} from 40 to 45 has eliminated any barrier to the full amount of this rise. Upon setting

$$\begin{aligned}v_{15}&=T_5+p_{15}-U_{15}=T_5+p_{15}-U_{25}+c_{24}-c_{14}\\&=T_5+p_{15}-p_{25}+v_{25}+c_{24}-c_{14}\\&=T_5+p_{15}-q_{25}+c_{24}-c_{14}=5,\end{aligned}$$

a lowering of v_{15} from 10 to 5 is noted, along with the derived formula, in panel 1 of 5β.

The minimum value

$$\begin{aligned}E&=(B-A_1-A_2)T_1+A_1U_{11}+A_2U_{21}+D_{15}v_{15}+D_{25}v_{25}\\&=75\times35+100\times60+25\times85+100\times5+20\times10=\$11{,}450\end{aligned}$$

is obtained for the dual functional. Using Table XI the relations

$$\begin{aligned}U_{11}&=T_1+c_{11}\\U_{21}&=T_1+p_{21}\end{aligned}$$

are used to simplify this expression to

$$\min E=BT_1+A_1c_{11}+A_2p_{21}+D_{15}v_{15}+D_{25}v_{25}.$$

Since

$$\begin{aligned}T_1&=q_{25}-c_{24}+p_{24}-c_{22}+p_{12}-c_{11},\\v_{15}&=p_{15}-q_{25}+c_{24}-c_{14},\\v_{25}&=p_{25}-q_{25},\end{aligned}$$

as noted in the table, the dual theorem and regrouping principles may be applied to yield

$$\begin{aligned}\max\pi=&-(B-A_1)c_{11}+A_2p_{21}-Bc_{22}+Bp_{12}-(B-D_{15})c_{24},\\&-D_{15}c_{14}+Bp_{24}+(B-D_{15}-D_{25})q_{25}+D_{15}p_{15}+D_{25}p_{25}.\end{aligned}$$

The programme of Table XII is thus ascertained.

TABLE XII

TRANSACTIONS FOR OPTIMUM PROGRAMME

Period		Receipts ($)	Disbursements ($)	Difference ($)
1	Sell $A_2 = 25$ tons of Product 2 at \$50	1,250		−1,250
	Buy $(B - A_1) = 100$ tons of Product 1 at \$25		2,500	
2	Sell $B = 200$ tons of Product 1 at \$35	7,000		2,000
	Buy $B = 200$ tons of Product 2 at \$25		5,000	
3	No transaction	—	—	—
4	Sell $B = 200$ tons of Product 2 at \$40	8,000		1,000
	Buy $(B - D_{15}) = 100$ tons of Product 2 at \$35		3,500	
	Buy $D_{15} = 100$ tons of Product 1 at \$35		3,500	
5	Sell $D_{25} = 20$ tons of Product 2 at \$55	1,100	—	9,700
	Sell $(B - D_{25} - D_{15}) = 80$ tons of Product 2 at \$45	3,600		
	Sell $D_{15} = 100$ tons of Product 1 at \$50	5,000		
	Total	25,950	14,500	11,450

Conclusion

The models and methods that have been presented can be extended in a variety of directions. For example, price variations on the purchasing side can be represented by adding positive and negative triangles to each pair of triplets in (26) and a new diagonal below the "dummy variables" so introduced in order to limit the amount that can be acquired at the initial price in any period. If quantities ordered beyond the stated limit require payment of a premium, no difficulty will be experienced. If, as is often the case, additional amounts can be purchased at a discount, further treatment will be required in order to ensure that the quantities purchased at a lower price do not take precedence over the required initial purchase amount in the process of optimization.

In some of the cases that have been treated it was observed that disbursements exceeded receipts in some periods. This naturally raises the question of liquidity considerations and credit limits relative to alternative opportunities. The usual form of discounting receipts and disbursements provides a method of ascertaining whether such business is worthwhile when compared with yields which might be secured by committing these resources to other endeavours. Such an analysis is, however, implicitly stated in terms of a "yes-no" model of choice. The "warehousing entrepreneur" wishes to decide whether to commit himself entirely to one line of endeavour. Rather simple adjustments permit some extensions by considering the periods when extra disbursements are required. In each cases the entrepreneur might be viewed as considering the

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alternative of abstaining from this business in particular periods. More generally, liquidity considerations and present-value considerations might be treated as constraints and optimum amounts of resource commitments determined accordingly.

These brief remarks must serve to indicate how the examples and methods discussed in this paper may be extended to encompass other aspects of the warehousing problem. Generalizations of a more fundamental type are also possible. As noted at the outset of this paper, one task of management science research is to develop and relate significant classes of models which comprehend a variety of managerial situations, to study their salient features and to evolve efficient methods of computation and analysis for each such class. This topic is too large to enter into here. Relations between the warehousing model and other classes such as transportation-type models have already been noted. Certain other kinds of models involving cumulative machine loading and scheduling through time can also be related to these models. Elaboration of this topic must await another occasion and the results of further research. The warehousing examples presented in this paper provide an important link in this chain of developments, covering a variety of managerial problems in both static and dynamic contexts.

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MEETINGS AND COURSES

MEETINGS FOR THE NEW YEAR are planned as follows: 11 January, The Earl of Halsbury on *The Assessment of New Inventions*; 7 February, Dr. H. F. Rance on *Objective and Subjective Factors in the Control of the Quality of Paper*. Sir Owen Wansbrough-Jones will be in the Chair for the January meeting, and Dr. T. E. Easterfield for the February meeting. The date for the March meeting has not yet been fixed. The subject will be *The Use of Automation in Operational Research*. Sir Charles Goodeve will be the Chairman, and among the speakers will be Dr. S. H. Hollingdale of the Royal Aircraft Establishment, D. G. Owen of the British Iron and Steel Research Association and speakers from Tube Investments Ltd.

The Society has been assisting the Northampton Polytechnic, London, E.C.1, in arranging a course of ten evening lectures and discussions on Operational Research to be held on Tuesdays from 10 January to 13 March. The course is intended for all who are interested in the application of scientific methods in industry.

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A MIXED STRATEGY IN ACTION

by

R. S. BERESFORD* AND M. H. PESTON†

DESPITE THE CONSIDERABLE current interest in the Theory of Games, it does not appear to have been used so far for the solution of many practical problems. An actual tactical problem, whose solution followed, although unconsciously, the lines suggested by the Theory of Games, may therefore be of interest. The setting is that of a military escort protecting a convoy of food lorries against guerilla attack.

A convoy of civilian food lorries used to travel daily along a jungle-fringed road in Malaya and was subject to occasional attack by Communist terrorists. The lorries were protected by a military escort of four armoured vehicles equipped with machine guns and carrying a small force of infantry which could be dismounted if necessary. There was wireless communication between the four vehicles and also back to their base camp

On any one day the terrorists could either: (*a*) stage a full-scale attack on the convoy, or (*b*) fire at the convoy to cause casualties and to deter the drivers of the food lorries from serving on the convoy again. Once committed to one of these courses of action it was virtually impossible for them to change to the other.

To satisfy various commercial interests, the convoy had to run at the same time each day so the courses open to the escort were restricted to the various possible deployments of the armoured vehicles along the convoy. One further limiting condition was that, for satisfactory control, there had to be at least one escort vehicle at each end of the convoy. The alternatives were therefore: (*a*) one vehicle at the front of the convoy and three vehicles at the rear; (*b*) one vehicle at the front, one at the rear and the other two distributed along the convoy either together or separately; (*c*) two vehicles at the front and two at the rear; (*d*) three vehicles at the front and one at the rear.

Thus with four strategies available to the convoy escort and two to the terrorists, there are eight outcomes to be evaluated. Against a full-scale terrorist attack, the convoy escort should preferably be concentrated and so placed that it tends to be carried towards the ambush position rather than away from it. Against terrorist sniping, though, the escort should be dispersed so that it can spot where the fire is coming from and return it quickly.

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It is difficult to make any quantitative assessment of the value of each outcome, especially in terms of cardinal numbers (e.g., units of utility), but the following ranking, derived from the likelihood of depriving the terrorists of success, seems fairly reasonable:

		Courses of Action Open to the Terrorists	
		Full-scale Attack	Sniping
Courses of action open to the convoy escort	One in front and 3 behind	Best	Worst
	Distributed	Worst	Best
	Two in front and 2 behind	Medium	Worst
	Three in front and 1 behind	Worst	Worst

Naturally, the terms "Best", "Medium" and "Worst" are to be interpreted solely as first, second and third preferences.

This ranking, of course, applies only in the first place to the escort and, to make any progress towards the computation of optimum strategies, it is necessary to consider also the enemy's evaluation of the various possible situations. Clearly, the interests of the two sides in these circumstances are diametrically opposed so that the game must essentially be zero-sum. In the ordinal case this seems to imply that the highest point on the escort's scale of value should correspond to the lowest point on the terrorist's scale and vice versa, and that intermediate points should correspond in the same way too. In other words, the m'th point counting from the top of one scale should correspond to the m'th point counting from the bottom of the other. The pay-off matrix from the enemy point of view will then be the same as the escort's pay-off matrix with "Best" substituted for "Worst" and "Worst" for "Best" throughout.

Obviously, from the point of view of the escort, the third and fourth rows are dominated by the first two so that their pay-off matrix may be simplified to:—

	Full-scale Attack	Sniping	Maximin
One in front and 3 behind	Best	Worst	Worst
Distributed	Worst	Best	
Minimax	Best		—

Maximin is defined as the maximum of the row minima. Minimax is the minimum of the column maxima. In this particular example both row minima are the same, "Worst", and both column maxima are the same, "Best", but that is not always the case.

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It can now be seen that there are no equilibrium points or saddle points so that the optimum solutions are mixed strategies both for the escort and for the terrorists. Unfortunately, it is not easy to determine precisely what probabilities to allot to the alternative strategies, indeed the application of probabilities appears to imply immediately the existence of a cardinal preference schedule. In this case, however, to allot equal probability to each of the two escort strategies seems as good an approximation as any other.

In the real situation, the escort commander, who had never heard of the Theory of Games, decided that his two best *individual* courses of action were the "one in front and 3 behind" and the "distributed" deployments, though neither in itself would be entirely satisfactory against both forms of terrorist action. It was important that the terrorists should not be able to learn beforehand which course the escort would adopt. The escort commander also wanted to eliminate his own personal preference from the choice and therefore adopted the following method. Each morning, as the convoy was forming up, he would ask one of the other members of the escort to decide which hand held behind his back contained a blade of grass. Then, depending on the reply, the escort would be deployed either in the "distributed" or the "one in front and 3 behind" form. This solution is, of course, that proposed by the Theory of Games.

It would be nice to say that this policy was the cause of a resounding success. In truth, though the terrorist attacks did slacken off, it is impossible to tell whether this was due to the improved general situation or to the particular tactics of the escort. However, the fact that, in an actual situation, the escort commander should adopt what is in reality a Theory of Games mixed strategy, seems to indicate that there should be no great difficulty in the further application of game-theory techniques to other minor tactical problems. It would appear that mixed strategies might well be employed in those types of operations which have to be carried out frequently but whose detailed execution can be varied. Alternatively they could be used when many small but similar operations have to be carried out simultaneously over a large area.

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General Post

We have been asked to include in the Quarterly a note of movements of operational research workers. We give below a list of new appointments which have come to our knowledge as having been taken up during the past 12 months. We should be grateful if correspondents would keep us advised of movements in the future.

E. C. Williams (D.O.R. Admiralty), to Scientific Adviser, Intelligence, Ministry of Defence. (December 1954.)

Dr. E. Lee, D.O.R. Admiralty in succession to E. C. Williams.

J. Stringer (London Transport), to Operational Research Section in the Chief Engineer's Department, Central Electricity Authority. (April.)

R. T. Eddison (B.I.S.R.A.), to Manager, Planning and Methods Department, Navy, Army and Air Force Institutes. (July.)

M. D. J. Brisby (B.I.S.R.A.), to Iron and Steel Board. (August.)

Dr. T. U. Matthew (Birmingham University), to T.I. (Group Services) Ltd. (August.)

R. H. Collcutt, Operational Research Section, B.I.S.R.A., in succession to R. T. Eddison. (September.)

B. D. Hankin, Assistant to Director of Research, London Transport Executive, in succession to J. Stringer. (October.)

H. A. Sergeant (War Office), to Deputy Scientific Adviser, Supreme Allied Commander in Europe.

S. L. Cook (National Coal Board), has been appointed Operational Research Manager, Richard Thomas & Baldwins Ltd. and will take up his appointment in 1956.

Contributions to the Quarterly

During last year the President of the Society appealed to members to submit contributions on industrial applications of operational research. A moderate response resulted which the editors would very much like to see maintained and increased.

C.G.O.S.—A Correction

In our report of the July conference of the Commission Générale d'Organization Scientifique (*Operat. Res. Quart.* **6** (3), 116) the last word was left off the title of this body. We regret and apologize for the inadvertence.

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